THE

LONDON, EDINBURGH, AND DUBLIN

PHILOSOPHICAL MAGAZINE

AND

JOURNAL OF SCIENCE.

"Nec aranearum sane textus ideo melior quia ex se fila gignunt, nec noster vilior quia ex alienis libamus ut apes." Just. Lips. *Polit.* lib. i. cap. 1. Not.

VOL. XXV.

CONTENTS OF VOL. XXV.

(THIRD SERIES.)

NUMBER CLXIII.—JULY, 1844.

NUMBER CLXIV.—AUGUST.

NUMBER CLXV.—SEPTEMBER.

NUMBER CLXVI.—OCTOBER.

NUMBER CLXVII.—NOVEMBER.

NUMBER CLXVIII.—DECEMBER.

NUMBER CLXIX.—SUPPLEMENT TO VOL. XXV.

ERRATA.

Page 18, note ‡, for p. 18 read p. 154.
Corrections in the Translation of M. Wartmann's paper will be found
at p. 398.

I. *On Tithonized Chlorine. By* JOHN WILLIAM DRAPER, *M.D., Professor of Chemistry in the University of New York.*

[The following paper was read at the meeting of the British Association, held at Cork last year (1843). I have added to it, in an Appendix, some further observations subsequently made.—J. W. D.]

CHLORINE gas, which has been exposed to the daylight or to sunshine, possesses qualities which are not possessed by chlorine which has been made in the dark.

This is shown by the circumstance, that chlorine which has been exposed to the sunshine has obtained from that exposure the property of speedily uniting with hydrogen gas; a property not possessed by chlorine which has been made and kept in the dark.

This quality gained by the chlorine arises from its having absorbed tithonic rays corresponding in refrangibility to the indigo. It is not a transient, but apparently a permanent property, the rays so absorbed becoming latent, and the effect lasting for an unknown period of time. The facts which I shall proceed to describe will be interesting to chemists, because they plainly lead us to suspect that the descriptions we have of the properties of all elementary and compound bodies are either inaccurate or confused. These properties are such as bodies exhibit after they have been exposed to the light; we still require to know what are the properties they possess before exposure to such influences.

Natural philosophers will also find an interest in these phænomena, for they finally establish for the tithonic rays two important facts,—1st, that those rays are absorbed by ponderable bodies; and 2nd, that they become latent after the manner of heat. Some years ago I endeavoured to prove that

these things held for a compound substance the iodide of silver (Phil. Mag. Sept. 1841).

For reasons which will be obvious as the description proceeds, I shall speak of chlorine which has been exposed to the beams of the sun, as *tithonized chlorine*.

I. *Description of the experiment.*

In two similar white glass tubes place equal volumes of chlorine, which has been made from peroxide of manganese and muriatic acid by lamplight, and carefully screened from access of daylight. Expose one of the tubes to the full sunbeams for some minutes, or if the light be feeble, for a quarter of an hour: the chlorine which is in it becomes tithonized. Keep the other tube during this time carefully in a dark place; and now, by lamplight, add to both equal volumes of hydrogen gas. These processes are best carried on in a small porcelain or earthenware trough, filled with a saturated solution of common salt, which dissolves chlorine slowly; and to avoid explosions operate on limited quantities of the gases. Tubes that are eight inches long and half an inch in diameter will answer very well. The two tubes now contain the same gaseous mixture, and only differ in the circumstance that one is tithonized and the other not. Place them therefore side by side before a window, through which the entrance of daylight can be regulated by opening the shutter; and now, if this part of the process is conducted properly, it will be seen that the tithonized chlorine commences to unite with the hydrogen, and the salt water rises in that tube. But the untithonized chlorine shows no disposition to unite with its hydrogen, and the liquid in its tube remains motionless for a long time. Finally, as it becomes slowly tithonized by the action of the daylight impinging on it, union at last takes place. From this, therefore, we perceive that chlorine which has been exposed to the sun will unite promptly and energetically with hydrogen; but chlorine that has been made and kept in the dark shows no such property.

As I doubt not this remarkable experiment will be repeated by chemists, I will add that the only point to which attention in particular is to be given, is in the final exposure to the light. This must not be too feeble, or the action will be tedious; but the direct sunbeam must be sedulously excluded, or an explosion will result. A room illuminated by one small window, looking to the north, answers very well. It need scarcely be added that care must be taken that both tubes are illuminated alike.

II. *The change in the chlorine is not transient.*

Now it might be supposed that this apparent exaltation of the electro-negative properties of the chlorine is only a transient thing which would speedily pass away, the gas reverting to its original untithonized condition.

To show that this is not so, tithonize some chlorine in a tube as before. Place it for an hour or two in the dark along with the tube of untithonized chlorine, with which it is to be compared; then to both add hydrogen. Expose them as in the former experiment to the daylight, and the result will turn out as before, the tithonized chlorine forming muriatic acid at once, and the untithonized refusing to do so.

This, therefore, shows that the change which the sunbeams impress upon chlorine is to a certain extent a permanent change, and, unlike a calorific effect, it does not spontaneously and rapidly pass away.

III. *There are two stages in the phænomenon.*

Let us now proceed to make inquiry into the nature of the change thus impressed on the chlorine. This, I shall show, rests in the circumstance of the absorption of rays which correspond in refrangibility to the indigo, and which appear to become latent.

In a tube, over salt water, mix together equal volumes of untithonized chlorine and hydrogen gas. Expose it to the daylight, marking the time at which the exposure commences. Watch the level of the liquid in the tube narrowly, and, though stationary for a considerable time, after a certain period has elapsed it will be seen on a sudden to start and commence rising. Observe now how far it will rise during a period which is equal to the time that elapsed between the first exposure and the beginning of the rise, and it will be seen that one-fourth or half the gases will disappear.

It is obvious that from the first moment of exposure the rays must have been exerting their influences on the mixture. As will presently be proved, absorption has been all along taking place. There are, therefore, two distinct phænomena exhibited by this experiment. There is a period during which, though large quantities of the dark rays are disappearing, no visible change is produced; there is a second period, during which absorption is accompanied by a remarkable chemical effect, the production of muriatic acid. From these things we gather that a definite amount of the tithonic rays must disappear and become latent before muriatic acid can form. The phænomenon is not unlike that of the disappearance of a definite quantity of heat in the passage of ice into the condition of water.

A mixture of chlorine and hydrogen does not, therefore, instantly give rise to the production of muriatic acid on exposure to the light, but as a preliminary condition a certain definite amount of absorption must take place.

Now if this were a mere molecular disturbance, such as might be brought about by the action of heat, we should expect to find it transient and speedily passing away. Such, however, is far from being the case. As with simple chlorine, so with this mixture, after it has been tithonized it loses its quality very slowly. I have observed that after a week or more has elapsed since it was first exposed to the light, it commences to contract when placed in a feeble gleam.

IV. *Rays are absorbed in producing this change.*

I have thus far assumed that the rays which bring about these changes are absorbed; the following is the proof which I have to offer:—

Over a tube half an inch in diameter and six inches long, closed at its upper extremity and open at its lower, invert a jar of the same length and one inch and a half in diameter. Fill the tube and the jar at the salt water trough, about two-thirds full, with the same mixture of chlorine and hydrogen. Expose them to diffuse daylight. Now it is clear that no rays can gain access to the tube, except after having passed through the gaseous mixture in the jar. After a certain space of time the level of the liquid in the jar commences to rise, but that in the tube will remain much longer wholly stationary.

It therefore appears that a beam which has passed through a mixture of chlorine and hydrogen has lost, to a great extent, the quality of bringing about the union of a second portion of the mixed gases through which it may be caused to traverse. The active rays have been absorbed; they disappear from the beam, and are lost in producing their first effect.

A beam of light becomes detithonized in producing a chemical effect; the beam, as well as the medium on which it acts, becomes changed. I have a series of results which proves that this takes place for a great variety of compound bodies.

V. *It is the indigo ray which is absorbed.*

As has been said, it is a ray which corresponds in refrangibility to the indigo which produces these results.

In a small porcelain trough I inverted, side by side, ten tubes, each of which was three inches long and one-third of an inch in diameter, the trough being filled with salt water. I passed into each tube a certain quantity of untithonized chlorine and hydrogen. A beam of the sun, being directed by a heliostat into a dark room, was dispersed horizontally

by a flint glass prism, and the trough with its tubes so placed as to offer an exposure to the different coloured rays. The aperture which admitted the beam was about half an inch in diameter. For awhile no movement was observed in any of the tubes; but as soon as the preliminary absorption, previously described, was over, and the tithonization completed, the level of the liquid began to rise. In the red and in the orange no movement could be perceived, in the violet only after a time; but first of all the tube that was immersed in the indigo light was in action, and exhibited finally a very rapid rise; this was soon followed by the tube that was in the space where the indigo and violet joined, then by that in the violet, and that in the blue; the tube in the green was next in order. The following Table gives the numerical results obtained by observing the time which elapsed before movement took place in each tube :—

Table I.

Name of ray.	Time.	Name of ray.	Time.
Extreme red	*	Indigo..............	1·50
Red and orange...	+100·00	Indigo and violet..	2·00
Yellow and green.	52·00	Violet..............	2·25
Green and blue...	4·00	Violet...............	5·00
Blue	2·33	Extreme violet ...	5·50

Many years ago M. Berard made experiments on the explosion of chlorine and hydrogen, and concluded from his results that it was brought about by the violet ray. This was at a time when the methods of making these experiments were less exactly known. It is a very easy matter to prove that in reality the indigo is the active ray, and that, from a maximum point which is in the indigo, but towards the blue, the effect gradually diminishes to each end of the spectrum.

The following Table gives the calculated approximate intensity of the chemical force for each ray, deduced from the foregoing experiment:—

Table II.

Name of ray.	Force.	Name of ray.	Force.
Extreme red	....	Indigo	66·60
Red and orange...	1·00	Indigo and violet..	50·00
Yellow and green.	1·90	Violet..............	44·40
Green and blue ...	25·00	Violet..............	20·00
Blue	42·90	Extreme violet ...	18·10

* Even after the longest exposure I had the means of giving it, no movement took place in the tube which was in the extreme red, and I am doubtful about that in the red and orange.

There is a great advantage which experiments conducted in this way possess over those which depend for their indication on the stains impressed on Daguerreotype plates or sensitive papers. In those cases we obtain merely a comparative contrast for different regions of the spectrum; in this we have absolute measures determined by a definite chemical effect and the rise of a liquid in a graduated tube; and from this we gain juster views of the true constitution of the spectrum. On studying the numbers in the foregoing table, or better still, if we project them, it will appear what an enormous difference there is in the chemical force of the different rays. In the experiment from which I have deduced this table, it appears that the force of the indigo ray exceeds that of the orange in a greater ratio than 66 to 1 ; and from the circumstances under which the experiment is made, this difference must be greatly underrated. There is always diffused light in the room coming from the intromitted beam, and this accelerates the rise in the less refrangible tubes; then again, it is impossible that the tube which gives the greatest elevation shall coincide mathematically with the maximum point and express the maximum effect.

From some estimates I have made, I am led to believe that in point of chemical force, for this mixture of chlorine and hydrogen, the indigo ray exceeds the red in a higher ratio than 500 to 1.

VI. *The action is positive from end to end of the spectrum.*

M. Becquerel found, that for a Daguerreotype plate the red, the orange, and the yellow rays possess the quality of continuing the action begun by the more refrangible colours; he therefore names these "*rayons continuateurs.*" For the same compound I found that those rays, acting horizontally with the diffused daylight, exerted a negative agency. It is therefore desirable to understand whether, with respect to the gases now under consideration, the lesser refrangible rays exert anything in the way of an action of depression or hindrance to union. By direct experiment I found that this was not the case, the action being positive from end to end of the spectrum. This can be shown by removing the tubes, after they have been in the spectrum for an hour or two, into the gleams of daylight. One by one they exhibit after a time a rise, the order being the green first, then the yellow and the orange, and at last the red. And if at the same time a tube which has been kept in the dark be exposed along with them, they will all rise before it, showing that tithonization had set in and been going on in them all; that it had been more active

in the green than in the yellow, in the yellow than in the orange, in the orange than in the red; and, had the exposure to the spectrum been long enough, the liquid in every one of the tubes would have risen.

VII. *The indigo ray forms the muriatic acid as well as produces the preliminary tithonization.*

It only now remains to inquire, whether the rays which cause the production of the muriatic acid are those which effect the tithonization of the chlorine; in other words, whether the first stage of the process is brought about by the same agent which carries on the second. The experiment which I have just described shows that tithonization is most actively produced by the indigo ray, and it is easy to show that it is the same ray which carries on the second part of the process; for, if before placing the tubes in the prismatic spectrum we tithonize them in the daylight, so that the liquid has just commenced to rise in each, and then expose them to the spectrum, it will be found that the tube in the indigo rises most rapidly, and the others in the order stated before. Therefore we perceive that the same ray commences, carries on, and completes the process.

Few substances can exceed in sensitiveness to light a mixture of chlorine and hydrogen previously tithonized. Brought into the obscure daylight of a gloomy chamber, it is remarkable how promptly the level of the liquid in the tube rises; how, when the shutters are successively thrown open, the action becomes more and more energetic; and how, in an instant, it stops when the instrument is shaded by a screen.

I have not recorded in this communication a multitude of experiments of detail which go to support the conclusions here drawn, and which will be published at a proper time. It has been my object on this occasion to call attention to the fact, that chlorine, an elementary body, undergoes a change after exposure to the light; a change which appears to produce an exaltation of its electro-negative properties, as is shown by its power of uniting more energetically with hydrogen. This change must not be confounded with those transient elevations of activity due to increased temperature, inasmuch as this is more permanent in its character. It arises from the absorption of rays which exist most abundantly in the indigo space of the spectrum. That the phænomenon is due to a true absorption is fully shown in the circumstance, that a beam which has produced this effect has lost the quality of ever after producing a similar result. This is borne out by what we ob-

serve to take place when a feeble light falls on a mixture of chlorine and hydrogen which has been prepared in the dark. A certain space of time elapses before any formation of muriatic acid occurs, during which the absorption in question is going on; and when that is completed, and the mixture is tithonized, union of the gases begins and muriatic acid forms. From end to end of the spectrum the action is positive, and differs only in intensity; but this difference in intensity opens before us new views of the constitution and character of the solar beam.

University of New York,
June 20, 1843.

The foregoing paper was written almost a year ago, and since that time I have made several new observations corroborative of the results given.

Chlorine is not the only elementary substance in which the rays produce a change. In his chapter on phosphorus, Berzelius remarks, " Light produces in it (phosphorus) a peculiar change, of which the intimate nature is unknown; and which, so far as we can judge at present, does not alter its weight. It makes it take a red tint. This phænomenon occurs not only in a vacuum, even in that of a barometer, but also in nitrogen gas, in carburetted hydrogen, under water, alcohol, oil and other liquids. When we expose to the sunlight phosphorus dissolved in æther, oil, or hydrogen gas, it instantly separates under the form of red phosphorus; it undergoes very rapidly this modification in *violet light*, or in glass vessels of a violet colour. The light of the sun makes it easily enter into fusion in nitrogen gas, but it does not melt in hydrogen, and in the Torricellian vacuum it sublimes in the form of brilliant red scales." (Berzelius, *Traité*, tom. i. p. 258.)

Again, when speaking of phosphuretted hydrogen, he says, " Exposed to the influences of the direct solar light this gas is decomposed, a part of the phosphorus separates under the form of red phosphorus, and is deposited on the interior surface of the glass. If we cover the vessel which contains the gas imperfectly, no phosphorus is deposited on the covered spaces." (Ib. tom. i. p. 265.)

As Berzelius does not give these experiments as his own, and I do not know to whom we are indebted for them, I repeated some of them. Among other corroborative results, it appeared, that a piece of phosphorus of a pale or whitish colour, in a vessel filled with pure and dry carbonic acid gas, placed in the sunshine, rapidly exhibited the phænomenon in question. Eventually the phosphorus became of a deep blood-

red colour, and on the sides of the glass towards the light, feathery crystals formed, the tint of which bore a close resemblance to that of the red prussiate of potash.

Since the invention of the tithonometer, I have been able to observe more closely the habitudes of chlorine. In the description given of that instrument in the December number (1843) of this Journal, it is recommended to cast aside the first observation, because it never gives an accurate estimate of the true effect. When a mixture of chlorine and hydrogen is exposed, muriatic acid does not immediately form; but a preliminary tithonization is necessary, and then at the end of a certain period contraction begins to take place.

A tithonometer exposed to the daylight is much too powerfully affected to allow of the successive stages of change to be distinctly made out; the preliminary tithonization is accomplished so rapidly, that the indications of it are merged and lost in the contraction which instantly follows. It is necessary therefore that we should operate with a *small* lamp-flame.

To such a flame I exposed a mixture of chlorine and hydrogen, and marked the number of seconds which elapsed before contraction, arising from the production of muriatic acid, took place. The first indications of movement occurred at the close of 600 seconds.

The index then moved through the first degree in 480 seconds

...	...	...	second	...	165	...
...	...	...	third	...	130	...
...	...	...	fourth	...	95	...
...	...	...	fifth	...	93	...
...	...	...	sixth	...	93	...

and continued to move with regularity at the same rate.

These observations, therefore, prove that a very large amount of radiant matter is absorbed before chemical combination takes place; and that in the case of chlorine and hydrogen the total action is divisible into two periods; the first during which a simple absorption is taking place without a chemical effect, the second during which absorption is attended with the production of muriatic acid.

The facts which I am endeavouring to set forth prominently in this communication are,—1st, the preliminary tithonization just discussed; and 2nd, the persistent character of the change impressed upon chlorine when it has been exposed to the sun, an effect wholly unlike a calorific effect, which would soon disappear.

By resorting to the tithonometer we obtain information equally distinct upon the second point, that the preliminary

tithonization is not a transient effect which at once passes away, but is, on the contrary, a persistent change.

I tithonized the chlorine and hydrogen contained in the instrument, and kept it in the dark for ten hours. On exposure to the lamp rays it moved after a few seconds, showing, therefore, that the change which had been impressed on the chlorine was not lost. In the former case 600 seconds had elapsed before any movement was visible.

When, however, we remember that the invisible images on Daguerreotype plates, and even photographic impressions on surfaces of resin, and probably all other similar changes are slowly effaced, it would be premature to conclude that tithonized chlorine does not revert to its original condition. I have sometimes thought that there were in several of my experiments indications that this was taking place, but would not be understood to assert it positively. Whether it be so or not, one thing is certain, that the taking on of this condition and the loss of it is a very different affair from any transient exaltation of action due to a temporary elevation of temperature, or the contrary effect produced by cooling.

April 26, 1844.

II. *On Quaternions; or on a new System of Imaginaries in Algebra*[*]. *By* Sir WILLIAM ROWAN HAMILTON, *LL.D., P.R.I.A., F.R.A.S., Hon. M. R. Soc. Ed. and Dub., Hon. or Corr. M. of the Royal or Imperial Academies of St. Petersburgh, Berlin, Turin, and Paris, Member of the American Academy of Arts and Sciences, and of other Scientific Societies at Home and Abroad, Andrews' Prof. of Astronomy in the University of Dublin, and Royal Astronomer of Ireland.*

1. LET an expression of the form

$$Q = w + i\,x + j\,y + k\,z$$

be called a *quaternion*, when w, x, y, z, which we shall call the four *constituents* of the quaternion Q, denote any real quantities, positive or negative or null, but i, j, k are symbols of three imaginary quantities, which we shall call *imaginary units*, and shall suppose to be unconnected by any linear relation with each other; in such a manner that if there be another expression of the same form,

$$Q' = w' + i\,x' + j\,y' + k\,z',$$

the supposition of an equality between these two quaternions,

$$Q = Q',$$

---

[*] A communication, substantially the same with that here published, was made by the present writer to the Royal Irish Academy, at the first meeting of that body after the last summer recess, in November 1843.

shall be understood to involve four separate equations between their respective constituents, namely, the four following,

$$w = w', \quad x = x', \quad y = y', \quad z = z'.$$

It will then be natural to define that the *addition* or *subtraction* of quaternions is effected by the formula

$$Q \pm Q' = w \pm w' + i(x \pm x') + j(y \pm y') + k(z \pm z');$$

or, in words, by the rule, that *the sums or differences of the constituents of any two quaternions, are the constituents of the sum or difference of those two quaternions themselves.* It will also be natural to define that the product $Q Q'$, of the multiplication of Q as a multiplier into Q' as a multiplicand, is capable of being thus expressed:

$$\begin{aligned}
Q Q' = {} & w w' + i w x' + j w y' + k w z' \\
& + i x w' + i^2 x x' + i j x y' + i k x z' \\
& + j y w' + j i y x' + j^2 y y' + j k y z' \\
& + k z w' + k i z x' + k j z y' + k^2 z z';
\end{aligned}$$

but before we can reduce this product to an expression of the quaternion form, such as

$$Q Q' = Q'' = w'' + i x'' + j y'' + k z'',$$

it is necessary to fix on quaternion-expressions (or on real values) for the nine squares or products,

$$i^2, \ i j, \ i k, \ j i, \ j^2, \ j k, \ k i, \ k j, \ k^2.$$

2. Considerations, which it might occupy too much space to give an account of on the present occasion, have led the writer to adopt the following system of values or expressions for these nine squares or products:

$$i^2 = j^2 = k^2 = -1; \quad \cdot \ \cdot \ \cdot \ \cdot \ \cdot \ \cdot \ \text{(A.)}$$
$$i j = k, \ j k = i, \ k i = j; \quad \cdot \ \cdot \ \cdot \ \cdot \ \cdot \ \text{(B.)}$$
$$j i = -k, \ k j = -i, \ i k = -j; \quad \cdot \ \cdot \ \cdot \ \cdot \ \text{(C.)}$$

though it must, at first sight, seem strange and almost unallowable, to define that the product of two imaginary factors in one order differs (in sign) from the product of the same factors in the opposite order ($j i = -i j$). It will, however, it is hoped, be allowed, that in entering on the discussion of a new system of imaginaries, it may be found necessary or convenient to surrender *some* of the expectations suggested by the previous study of products of real quantities, or even of expressions of the form $x + i y$, in which $i^2 = -1$. And whether the choice of the system of definitional equations, (A.), (B.), (C.), has been a judicious, or at least a happy one, will probably be judged by the event, that is, by trying whether those equations conduct to results of sufficient consistency and elegance,

3. With the assumed relations (A.), (B.), (C.), we have the four following expressions for the four constituents of the product of two quaternions, as functions of the constituents of the multiplier and multiplicand:

$$\left. \begin{aligned}
w'' &= w\,w' - x\,x' - y\,y' - z\,z', \\
x'' &= w\,x' + x\,w' + y\,z' - z\,y', \\
y'' &= w\,y' + y\,w' + z\,x' - x\,z', \\
z'' &= w\,z' + z\,w' + x\,y' - y\,x'.
\end{aligned} \right\} \quad \ldots \ldots \quad \text{(D.)}$$

These equations give

$$w''^2 + x''^2 + y''^2 + z''^2 = (w^2 + x^2 + y^2 + z^2)\,(w'^2 + x'^2 + y'^2 + z'^2);$$

and therefore

$$\mu'' = \mu\,\mu', \quad \ldots \ldots \ldots \quad \text{(E.)}$$

if we introduce a system of expressions for the constituents, of the forms

$$\left. \begin{aligned}
w &= \mu \cos \theta, \\
x &= \mu \sin \theta \cos \phi, \\
y &= \mu \sin \theta \sin \phi \cos \psi, \\
z &= \mu \sin \theta \sin \phi \sin \psi,
\end{aligned} \right\} \quad \ldots \ldots \quad \text{(F.)}$$

and suppose each μ to be positive. Calling, therefore, μ the *modulus* of the quaternion Q, we have this theorem: that *the modulus of the product* Q″ *of any two quaternions* Q *and* Q′, *is equal to the product of their moduli.*

4. The equations (D.) give also

$$w'\,w'' + x'\,x'' + y'\,y'' + z'\,z'' = w\,(w'^2 + x'^2 + y'^2 + z'^2),$$
$$w\,w'' + x\,x'' + y\,y'' + z\,z'' = w'\,(w^2 + x^2 + y^2 + z^2);$$

combining, therefore, these results with the first of those equations (D.), and with the trigonometrical expressions (F.), and the relation (E.) between the moduli, we obtain the three following relations between the angular co-ordinates $\theta\,\phi\,\psi$, $\theta'\,\phi'\,\psi'$, $\theta''\,\phi''\,\psi''$ of the two factors and the product:

$$\left. \begin{aligned}
\cos\theta'' &= \cos\theta\cos\theta' - \sin\theta\sin\theta'\,(\cos\phi\cos\phi' + \sin\phi\sin\phi'\cos(\psi - \psi')), \\
\cos\theta &= \cos\theta'\cos\theta'' + \sin\theta'\sin\theta''(\cos\phi'\cos\phi'' + \sin\phi'\sin\phi''\cos(\psi' - \psi'')), \\
\cos\theta' &= \cos\theta''\cos\theta + \sin\theta''\sin\theta\,(\cos\phi''\cos\phi + \sin\phi''\sin\phi\cos(\psi'' - \psi)).
\end{aligned} \right\} \text{(G.)}$$

These equations (G.) admit a simple geometrical construction. Let $x\,y\,z$ be considered as the three rectangular co-ordinates of a point in space, of which the radius vector is $= \mu \sin\theta$, the longitude $= \psi$, and the co-latitude $= \phi$; and let these three latter quantities be called also the *radius vector*, the *longitude* and the *co-latitude of the quaternion* Q; while θ shall be called the *amplitude* of that quaternion. Let R be the point where the radius vector, prolonged if necessary, intersects the spheric surface described about the origin of co-ordinates with a radius = unity, so that ϕ is the co-latitude and

ψ is the longitude of R; and let this point R be called the *representative point* of the quaternion Q. Let R′ and R″ be, in like manner, the representative points of Q′ and Q″; then the equations (G.) express that *in the spherical triangle* R R′ R″, *formed by the representative points of the two factors and the product* (in any multiplication of two quaternions), *the angles are respectively equal to the amplitudes of those two factors, and the supplement of the amplitude of the product* (to two right angles); in such a manner that we have the three equations:

$$R = \theta, \quad R' = \theta', \quad R'' = \pi - \theta''. \quad . \quad . \quad . \quad \text{(H.)}$$

5. The system of the four very simple and easily remembered equations (E.) and (H.), may be considered as equivalent to the system of the four more complex equations (D.), and as containing within themselves a sufficient expression of the rules of multiplication of quaternions; with this exception, that they leave undetermined the *hemisphere* to which the point R″ belongs, or the *side* of the arc R R′ on which that *product-point* R″ falls, after the *factor-points* R and R′, and the amplitudes θ and θ' have been assigned. In fact, the equations (E.) and (H.) have been obtained, not immediately from the equations (D.), but from certain combinations of the last-mentioned equations, which combinations would have been unchanged, if the signs of the three functions,

$$y z' - z y', \quad z x' - x z', \quad x y' - y x',$$

had all been changed together. This latter change would correspond to an alteration in the assumed conditions (B.) and (C.), such as would have consisted in assuming $ij = -k$, $ji = +k$, &c., that is, in taking the cyclical order kji (instead of ijk), as that in which the product of any two imaginary units (considered as multiplier and multiplicand) is equal to the imaginary unit following, taken positively. With this remark, it is not difficult to perceive that *the product-point* R″ *is always to be taken to the right, or always to the left of the multiplicand-point* R′, *with respect to the multiplier-point* R, according as the semiaxis of $+z$ is to the right or left of the semiaxis of $+y$, with respect to the semiaxis of $+x$; or, in other words, according as the positive direction of rotation in longitude is to the right or to the left. This *rule of rotation,* combined with the *law of the moduli* and with the *theorem of the spherical triangle,* completes the transformed system of conditions, connecting the product of any two quaternions with the factors, and with their order.

<div align="center">[To be continued.]</div>

III. *Comparative Analysis of Recent and Fossil Bones.*
By J. MIDDLETON, *Esq.*, *F.G.S.*

To Richard Phillips, Esq.

DEAR SIR,

HAVING lately devoted some time and attention to the analyses of bones, both recent and fossil, I trust some of the results at which I have arrived may not be unacceptable to the readers of the Philosophical Magazine. I took up the subject with the view of ascertaining, if possible, the law by which fluoride of calcium becomes augmented or developed in fossil bones, as, should this be established, an important step would, I conceived, be thereby made towards the ascertainment of geological time.

That fossil bones contain fluoride of calcium in greater quantity than recent ones is a fact which has long been known, though heretofore not adequately explained. One theory proposed to account for it on the hypothesis that the source of fluorine in animals is their food, and that that in former times must have contained a greater quantity of the substance than it now does, and thus the bones of animals then living came to possess it in a higher proportion. To this theory, however, I felt unable to consent, as, in order to entitle it to credence, its supporters should, in my opinion, be prepared to show that fluoride of calcium is capable from its nature of occupying the place and discharging the functions of phosphate of lime in living bones, without detriment to their health and strength; as also that if a greater quantity of fluoride of calcium were present in the food of animals, a greater proportion would be absorbed. To assume these is to assume too much, the more especially as the general constancy and uniformity of nature is thereby opposed, who, having her own materials to select from and to work with from the beginning, is as little likely to deviate in the constituent elements of things as in the laws by which the things themselves are governed.

It is perhaps unnecessary to examine other explanations less generally received, though all equally exceptionable; suffice it to say that unsatisfied with them I was induced to seek for another. In this search it occurred to me that fluorine might exist in common water, and if so, that its accumulation in fossil bones would be found to be the result of infiltration, as with carbonate of lime, peroxide of iron, &c. In order to ascertain whether there was any foundation for this view, I examined the following substances and found them to contain fluorine, some in greater some in less proportion :—

1. Deposit in a chloride of lime vat.
2.   ...   water-conduit pipe of a coal mine.

3. Stalactitic deposit from the old red sandstone*.

4. Deposit in a wooden pipe for conducting water from a building.

5. Deposit in a kettle used solely for the boiling of water.

6. Portion of a vein of sulphuret of barytes from the old red sandstone above mentioned.

7. Fossil wood from Egypt, fossilized by infiltration of carbonate of lime.

8. Fossil wood from Egypt, fossilized by infiltration of silica†.

I no longer entertained a doubt as to the source of fluorine in recent bones and of its accumulation in fossil ones. The fact which my investigations also disclosed to me, viz. that fluorine is not confined to the bones of recent Mammalia, but exists also in those of birds and reptiles, as also in the shells of Mollusca, was also thus accounted for. Indeed the last fact is alone enough to prove that the source of fluorine must be as generally diffused as water is; while the reception of that substance by the organism being secured and placed beyond the dominion of tastes or caprice, seems to elevate in importance the function which it has to fulfil.

I shall now proceed to detail a few of the analyses made in prosecution of the object with which my investigations were begun. My inferences as to the geological bearing of the results have already been submitted to the Geological Society.

First. *Colossochylus atlas* of the Sewalic Hills. Entosternal bone of the sternum, Caut. and Falc.

	per cent.
Phosphate of lime	64·95
Carbonate of lime	22·36
Fluoride of calcium	11·68
Peroxide of iron	1·00

Second. Fossil ruminant of the Sewalic.

	per cent.
Phosphate of lime	78·00
Carbonate of lime	11·34
Fluoride of calcium	10·65
Peroxide of iron	trace

Third. A fossil horse of the Sewalic.

	per cent.
Phosphate of lime	58·46
Fluoride of calcium	11·24
Carbonate of lime	28·80
Peroxide of iron	·60

* Contained about 8 per cent. of fluoride of calcium.
† A minute trace of fluorine.

Fourth. Fossil camel of the Sewalic.

per cent.

Phosphate of lime . . 62·35
Carbonate of lime . 25·23
Fluoride of calcium . 11·16
Peroxide of iron . . ·76

Fifth*. Part of a fossil alligator. Sewalic.

per cent.

Phosphate of lime . . . . . 75·79
Carbonate of lime . . . . . 7·40
Phosphate and peroxide of iron 8·67
Fluoride of calcium . . . . 4·85
Carbonate of magnesia . . . 1·76
Silica . . . . . . ·. . · . 1·50

The above are analyses of a few of several specimens fur-
nished to me by Dr. Falconer, a gentleman whose love of
science and perseverance in its cause are only equalled by the
cordiality with which he encourages and assists others en-
gaged in scientific pursuits.

Sixth. Iguanodon of the Wealden.

per cent.

Phosphate of lime . . . . 35·35
Carbonate of lime . . . . 19·59
Fluoride of calcium . . . . 11·51
Insoluble silicates . . . . 8·75
Chloride of sodium . . . . 1·26
Soda . . . . 2·50
Magnesia and chloride of magnesium . 3·50
Alumina and peroxide of iron . . . 6·91
Organic . . . . 10·71

Seventh. Recent shells.

per cent.

Carbonate of lime . . . . 99·01
Chloride of sodium . . . . ·20
Fluoride of calcium†, tissue and loss . ·79

Eighth. Sea urchin of the miocene from Malta.

per cent.

Carbonate of lime . . 98·12
Chloride of sodium . ·48
Insoluble silicates . . ·80
Fluoride of calcium . ·55

For the interesting subjects, of which the following are ana-

* The state of this fossil differed essentially from that of the foregoing,
they being soft and friable, that hard and refractory, and having quite a
mineral character.

† The quantity too small for estimation.

lyses, I am indebted to the kindness and courtesy of the authorities of University College. It will be readily seen how important they were to the investigations with which I set out, as also how directly they bear upon the truth to which my investigations led me, viz. " that fluorine in fossil bones is a product of infiltration."

The first of these analyses, and the ninth in order, is that of a Greek skull, its age being about 2000 years, as indicated by a coin found under the jaw, and which, according to usage, had no doubt been placed in the mouth of the corpse previous to burial. The bone had so far assumed a fossil character as to be friable; easily pulverized in a mortar, and having a faintly pinkish tint, due to the presence of peroxide of iron. The following were found to be its constituents:—

	per cent.
Phosphate of lime	70·01
Carbonate of lime	10·34
Fluoride of calcium	5·04
Organic matter	9·97
Insoluble in acids	1·68
Soda and chloride of sodium	1·15
Phosphate of magnesia	1·34
Peroxide of iron, a small quantity	

Tenth. Skull of an Egyptian mummy.

	per cent.
Organic matter	38·50*
Phosphate of lime	50·76
Carbonate of lime	6·01
Fluoride of calcium	2·35
Phosphate of magnesia	1·14
Soda and chloride of sodium	1·12

Eleventh. Analysis of a portion of a skull lately recovered from the wreck of the Royal George. This bone had undergone but little change in appearance from its normal state, while it had all the tenacity of recent bone. It had, however, a slightly yellowish tinge, and the cells between the plates were charged with a white substance, which, when examined, was found to consist of chloride and oxide of magnesium. The following were its constituents:—

	per cent.
Organic matter	31·59
Phosphate of lime	50·58
Carbonate of lime	9·83

* I can only account for the large proportion of organic matter and the small proportion of carbonate of lime by supposing them to be results of the process of embalming.

	per cent.
Fluoride of calcium.	1·86
Soda	1·08
Chloride of sodium .	2·42
Magnesia and chloride of magnesium .	3·50

Twelfth.   Analysis of a portion of a recent skull.

	per cent.
Organic matter .	33·43
Phosphate of lime .	51·11
Carbonate of lime .	10·31
Fluoride of calcium*	1·99
Soda	1·08
Chloride of sodium .	·60
Magnesia and phosphate of magnesia†	1·67

It is perhaps unnecessary to add more to these analyses than the statement that they have been performed with great care, and that to these and congeneric inquiries I have devoted some months; while pursued as they were in the laboratory of University College, I had the advantage of most able advice and assistance‡.   I am, &c.,

London, June 7, 1844.   J. MIDDLETON.

IV. *On the Rectification and Quadrature of the Spherical Ellipse.   By the Rev.* JAMES BOOTH, *LL.D., M.R.I.A., Vice-Principal of, and Professor of Mathematics in Liverpool College* §.

IN the *livraison* of the *Journal de Mathématiques* published in September 1841, a paper is given on the quadrature of the spherical ellipse, but as the method there adopted, although the established one in inquiries of this nature, appears in the present instance somewhat complex, and as the author, M. Catalan, has confined himself to merely reducing the quadrature to the evaluation of a complete *elliptic function* of the *third* order, without noticing or appearing to be aware of the singular relation which exists between the lengths

* So far as an inference may be drawn from qualitative indications, the bone of a fœtus of 6½ months contains as great a proportion of fluoride as that of an adult; an interesting fact, and not, I believe, previously noticed.

† If none of the magnesia existed in the bone as phosphate, which there is much reason to doubt, the phosphate of lime would be increased about 1 per cent; the fluoride of calcium would be therefore proportionally diminished.

‡ [The results of M.M. Girardin and Preisser's analyses of ancient and fossil bones will be found in Phil. Mag. S. 3. vol. xxiv. p. 18.—EDIT.]

§ Communicated by the Author.

and areas of those curves, nor of the striking analogies which connect together the plane and spherical ellipse, an investigation of the same problem, conducted on different principles, and leading to some very curious results, may not be unacceptable to the mathematical reader.

The method here pursued is founded on two general theorems, which may be enunciated and proved as follows:—

II. Theorem (1.). *The area* A of any portion of a spherical surface bounded by a curve may be determined by the formula

$$A = \int_0^{2\pi} d\omega \int_0^{\rho} d\sigma \cdot [\sin \sigma], \quad . \quad . \quad . \quad (1.)$$

where σ is the arc of a great circle intercepted between a fixed point P which may be termed the *pole*, and any variable point s assumed within the curve on the surface of the sphere, ρ the spherical radius of the curve measured from the pole and passing through the point s, ω the angle, which the plane of the great circle passing through the points $P \, s$ makes with the fixed plane of a great circle passing through the pole P.

Let O be the centre of the sphere, P the pole, s the assumed point, P Q the great circle passing through them; through P let a great circle O P Q′ be drawn indefinitely near the former, $d\omega$ being the angle between those planes; through s let a plane be drawn perpendicular to O P, meeting the great circle O P Q′ in s'. Let a point u be assumed on the circle P Q indefinitely near to s,

Fig. 1.

and through u let a plane be drawn perpendicular to O P, meeting the great circle O P Q′ in u'; it is clear that the whole area to be determined is the sum of the indefinitely small trapezia, such as $s u \, s' u'$, into which the required portion of the spherical surface is thus divided. To compute the value of this elementary area, we have $s s' = \sin \sigma \, d\omega$, $s u = d\sigma$; hence the area of the trapezium $s u s' u' = d\omega \sin \sigma \, d\sigma$; and the whole area A round the pole P, and bounded by the curve, is therefore given by the formula

$$A = \int_0^{2\pi} d\omega \int_0^{\rho} d\sigma \, [\sin \sigma].$$

Integrating this expression between the limits ρ and 0,

$$A = \int_0^{2\pi} d\omega \, [1 - \cos \rho]. \quad . \quad . \quad . \quad . \quad (2.)$$

III. Theorem (2.) To find an expression for the length of a curve described on the surface of a sphere.

C 2
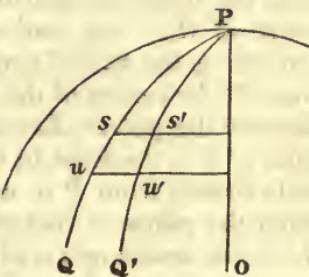
Let s and s' be two consecutive points on the curve, $P s$, $P s'$ two great circles passing through these points and the pole P, inclined to each other by the angle $d\omega$; through s let a plane be drawn perpendicular to O P, meeting the great circle $P s'$ in u; then ultimately $s s' u$ may be considered as a right-angled triangle. Hence $(s s')^2 = (s u)^2 + (s' u)^2$, but $s s' = d s$, $P s = \rho$, $s u = \sin \rho \, d\omega$, $s' u = d\rho$; or $(d s)^2 = (d\rho)^2 + (\sin \rho \, d\omega)^2$.

Fig. 2.

Integrating this expression and taking the limits ρ_1, ρ_0,

$$\mathrm{arc} = \int_{\rho_0}^{\rho_1} d\rho \left[1 + \left(\sin \rho \frac{d\omega}{d\rho} \right)^2 \right]^{\frac{1}{2}}. \quad . \quad . \quad (3.)$$

IV. Def. A spherical ellipse is the curve of intersection of a cone of the second degree with a concentric sphere.

Let 2α and 2β be the greatest and least vertical angles of the cone, which may be termed the *principal angles* of the cone, the origin of coordinates being placed at the common centre of the cone and sphere, and the real axe of the cone assumed as the axis of z meeting the surface of the sphere in the point P, the centre of the spherical ellipse, which point may be taken as the pole. Let the mean axe of the cone be in the plane of $x z$, the least in that of $y z$; ρ being the arc of a great circle drawn from P to any point Q of the ellipse, ω the angle which the plane of this circle makes with the plane of $x z$, in which the semiangle α of the cone is placed, then the polar equation of the spherical ellipse is

$$\frac{\cos^2 \omega}{\tan^2 \alpha} + \frac{\sin^2 \omega}{\tan^2 \beta} = \frac{1}{\tan^2 \rho}. \quad . \quad . \quad . \quad . \quad (4.)$$

To show this, through the point P let a tangent plane be drawn to the sphere, intersecting the cone in an ellipse, which for perspicuity may be termed the elliptic base of the cone; let the great circle passing through P and Q cut this ellipse in the central radius vector r, a and b being the semiaxes of this section, and c the radius of the sphere; then we have from the common equation of the ellipse,

$$\frac{\cos^2 \omega}{a^2} + \frac{\sin^2 \omega}{b^2} = \frac{1}{r^2},$$

ω being the angle between r and a, but $a = c \tan \alpha$, $b = c \tan \beta$, $r = c \tan \rho$; making these substitutions,

$$\frac{\cos^2 \omega}{\tan^2 \alpha} + \frac{\sin^2 \omega}{\tan^2 \beta} = \frac{1}{\tan^2 \rho}.$$
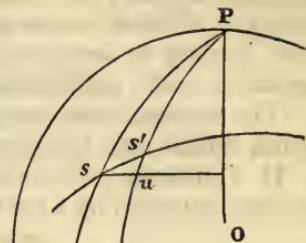
Now this equation may be written in the form

$$\frac{\cos^2 \omega}{\sin^2 \alpha}(1 - \sin^2 \alpha) + \frac{\sin^2 \omega}{\sin^2 \beta}(1 - \sin^2 \beta) = \frac{1 - \sin^2 \rho}{\sin^2 \rho},$$

or

$$\frac{\cos^2 \omega}{\sin^2 \alpha} + \frac{\sin^2 \omega}{\sin^2 \beta} = \frac{1}{\sin^2 \rho}, \quad . \quad . \quad . \quad . \quad (5.)$$

which is the equation of the spherical ellipse in another form.

V. Dividing (4.) by (5.), and reducing, there results

$$\cos \rho = \cos \alpha \; \frac{\sqrt{1 + \dfrac{\tan^2 \alpha}{\tan^2 \beta} \tan^2 \omega}}{\sqrt{1 + \dfrac{\sin^2 \alpha}{\sin^2 \beta} \tan^2 \omega}}. \quad . \quad . \quad . \quad (6.)$$

Substituting this value of $\cos \rho$ in (2.), integrating, and putting A for the area of a quadrant of the spherical ellipse (for, as the surface of this spherical ellipse evidently consists of four symmetrical quadrants, the length or area of one quadrant is one-fourth of the length, or of the area of the whole),

$$A = \frac{\pi}{2} - \cos \alpha \int_0^{\frac{\pi}{2}} d\omega \left[\frac{\sqrt{1 + \dfrac{\tan^2 \alpha}{\tan^2 \beta} \tan^2 \omega}}{\sqrt{1 + \dfrac{\sin^2 \alpha}{\sin^2 \beta} \tan^2 \omega}} \right]. \quad (7.)$$

VI. Now this definite integral is an elliptic function of the *third* order, as may be thus shown. Assume

$$\tan \omega = \frac{\tan \beta}{\tan \alpha} \tan \varphi, \quad . \quad . \quad . \quad . \quad . \quad (8.)$$

then

$$\frac{d\omega}{d\varphi} = \frac{\tan \alpha \tan \beta}{\tan^2 \alpha \cos^2 \varphi + \tan^2 \beta \sin^2 \varphi}. \quad . \quad . \quad (9.)$$

Introducing the relations established in (8.) and (9.) into (7.), the resulting equation becomes

$$\frac{\pi}{2} - \frac{\tan \beta}{\tan \alpha} \cos \alpha \int_0^{\frac{\pi}{2}} d\varphi \left[\frac{1}{\left\{ 1 - \left(\dfrac{\sin^2 \alpha - \sin^2 \beta}{\sin^2 \alpha \cos^2 \beta} \right) \sin^2 \phi \right\} \sqrt{1 - \left(\dfrac{\sin^2 \alpha - \sin^2 \beta}{\cos^2 \beta} \right) \sin^2 \phi}} \right]. \quad (10.)$$

VII. Let two right lines be drawn from the vertex of the cone in the plane of $x z$, or in the plane of the *principal angle* 2α, making equal angles ε with the real axe of the cone, so that

$$\cos \varepsilon = \frac{\cos \alpha}{\cos \beta}. \quad . \quad . \quad . \quad . \quad (11.)*$$

---

* The most accessible treatise to which I can refer the reader desirous of information on the subject of cones and spherical conics, is a translation of two Memoirs of Chasles, lately published by the Rev. Charles Graves,

These lines are termed *focals*, and the points in which they meet the surface of the spherical ellipse, are analogous to the foci of the plane ellipse.

Let e be the eccentricity of the plane elliptic base of the cone, then

$$e^2 = \frac{a^2 - b^2}{a^2} = \frac{\tan^2 \alpha - \tan^2 \beta}{\tan^2 \alpha} = \frac{\sin^2 \alpha - \sin^2 \beta}{\sin^2 \alpha \cos^2 \beta}; \quad (12.)$$

and by (11.) $\sin^2 \varepsilon = \dfrac{\sin^2 \alpha - \sin^2 \beta}{\cos^2 \beta}.$

Introducing the relations established in (11.) and (12.) into (10.), we find

$$A = \frac{\pi}{2} - \frac{\tan \beta}{\tan \alpha} \cos \alpha \int_0^{\frac{\pi}{2}} d\phi \left[\frac{1}{(1 - e^2 \sin^2 \phi) \sqrt{1 - \sin^2 \varepsilon \sin^2 \phi}} \right] (13.)$$

a complete elliptic function of the third order, whose parameter is of the *circular* form, as might be easily shown. This appears to be the simplest shape to which the quadrature of the spherical ellipse can be reduced, the parameter and modulus being the eccentricities of the plane and spherical ellipse respectively.

VIII. To find the length of an arc of the spherical ellipse.

In this case it will be found much simpler to integrate the equation (3.) with respect to ρ, instead of ω, the independent variable in the last problem; for this purpose, then, solving equation (5.), we find

$$\sin^2 \omega = \frac{\sin^2 \beta}{\sin^2 \rho} \left\{ \frac{\sin^2 \alpha - \sin^2 \rho}{\sin^2 \alpha - \sin^2 \beta} \right\} \quad \cdot \quad \cdot \quad \cdot \quad (14.)$$

$$\cos^2 \omega = \frac{\sin^2 \alpha}{\sin^2 \rho} \left\{ \frac{\sin^2 \rho - \sin^2 \beta}{\sin^2 \alpha - \sin^2 \beta} \right\}. \quad \cdot \quad \cdot \quad \cdot \quad (15.)$$

Differentiating (14.) with respect to ω and ρ,

$$\sin \omega \cos \omega \frac{d\omega}{d\rho} = \frac{-\sin^2 \alpha \sin^2 \beta \cos \rho}{\sin^3 \rho (\sin^2 \alpha - \sin^2 \beta)}.$$

Dividing this equation by the square root of the product of (14.) and (15.), we obtain

$$\frac{d\omega}{d\rho} = \frac{-\sin \alpha \sin \beta \cos \rho}{\sin \rho \sqrt{\sin^2 \alpha - \sin^2 \rho} \sqrt{\sin^2 \rho - \sin^2 \beta}}. \quad (16.)$$

Substituting this value of $\dfrac{d\omega}{d\rho}$ in the formula (3.) for the rec-

M.A., Fellow of Trinity College, Dublin, who has enriched his version with very valuable notes, and an appendix containing amongst other original matter a new theory of rectangular spherical coordinates, which is likely to become a powerful instrument of investigation in researches of this nature.

tification of the arc of a spherical curve, the resulting equation becomes

$$\text{arc} = \int_{\rho_0}^{\rho_1} d\rho \left[\frac{\sin\rho \sqrt{\cos^2\rho - \cos^2\alpha \cos^2\beta}}{\sqrt{\sin^2\alpha - \sin^2\rho} \sqrt{\sin^2\rho - \sin^2\beta}} \right], \quad . \quad (17.)$$

the arc being measured from the minor axis towards the major.

IX. Let s be the arc of a spherical quadrant, then

$$s = \int_{\beta}^{\alpha} d\rho \left[\frac{\sin\rho \sqrt{\cos^2\rho - \cos^2\alpha \cos^2\beta}}{\sqrt{\sin^2\alpha - \sin^2\rho} \sqrt{\sin^2\rho - \sin^2\beta}} \right]. \quad (18.)$$

This is a complete elliptic function of the third order, which may be reduced to the usual form in the following manner. Assume

$$\cos^2\rho = \frac{\sin^2\alpha \cos^2\phi + \sin^2\beta \sin^2\phi}{\tan^2\alpha \cos^2\phi + \tan^2\beta \sin^2\phi}, \quad . \quad . \quad (19.)$$

the limits of integration being changed from α and β to 0 and $\frac{\pi}{2}$, or (changing as well the order of integration as the sign) from $\frac{\pi}{2}$ to 0. Differentiating (19.) and introducing the relations assumed in it into (18.), there results the equation

$$s = \frac{\tan\beta}{\tan\alpha} \sin\beta \int_0^{\frac{\pi}{2}} d\phi \left[\frac{1}{(1 - e^2 \sin^2\phi) \sqrt{1 - \left(\frac{\sin^2\alpha - \sin^2\beta}{\sin^2\alpha}\right) \sin^2\phi}} \right] (20.)$$

X. Let γ denote the angle which the plane of one of the circular sections of the cone makes with the plane elliptic base, then it may be shown with little difficulty that

$$\cos\gamma = \frac{\sin\beta}{\sin\alpha}; \quad . \quad . \quad . \quad . \quad . \quad . \quad (21.)$$

or $\sin^2\gamma = \frac{\sin^2\alpha - \sin^2\beta}{\sin^2\alpha}$. Introducing this relation into (20.),

$$s = \frac{\tan\beta}{\tan\alpha} \sin\beta \int_0^{\frac{\pi}{2}} d\phi \left[\frac{1}{(1 - e^2 \sin^2\phi) \sqrt{1 - \sin^2\gamma \sin^2\phi}} \right] \quad (22.)$$

a complete elliptic function of the third order, whose parameter is also of the *circular* form.

XI. Let α' and β' be the principal semiangles of the *supplemental cone*[*], and s' the length of a quadrant of the spherical ellipse, the curve of intersection of this cone with the sphere, then

---

[*] A cone is said to be supplemental to another when their *principal angles* are supplemental.

$$s' = \frac{\tan\beta'}{\tan\alpha'}\sin\beta'\int_0^{\frac{\pi}{2}} d\phi \left[\frac{1}{\{1-e'^2\sin^2\phi\}\sqrt{1-\sin^2\gamma'\sin^2\phi}}\right]. \quad (23.)$$

Now as the cones are supplemental,

$$\alpha + \beta' = \frac{\pi}{2}, \quad \beta + \alpha' = \frac{\pi}{2}, \quad \sin\beta' = \cos\alpha, \quad \sin\alpha' = \cos\beta;$$

hence $\dfrac{\tan\beta'}{\tan\alpha'} = \dfrac{\tan\beta}{\tan\alpha}, \; e' = e, \; \sin\gamma' = \sin\varepsilon. \quad . \quad . \quad (24.)$

Making these substitutions in (23.), we find

$$s' = \frac{\tan\beta}{\tan\alpha}\cos\alpha\int_0^{\frac{\pi}{2}} d\phi \left[\frac{1}{\{1-e^2\sin^2\phi\}\sqrt{1-\sin^2\varepsilon\sin^2\phi}}\right]. \quad (25.)$$

Adding this equation to (13.), we obtain the very simple relation [*]

$$\mathrm{A} + s' = \frac{\pi}{2}; \quad . \quad . \quad . \quad . \quad . \quad . \quad (26.)$$

or taking the whole surface S of the spherical conic, and the whole circumference Σ' of the supplemental conic, introducing c the radius of the sphere, we obtain the remarkable theorem

$$\mathrm{S} + c\,\Sigma' = 2\,c^2\pi. \quad . \quad . \quad . \quad . \quad . \quad (27.)$$

Now $c\,\Sigma'$ is twice the lateral surface of the supplemental cone, measured in one direction only from the centre, and may be put equal to $2\,\mathrm{L}'$, hence we deduce that

XII. *The spherical base of any cone, together with twice the lateral surface of the supplemental cone, is equal to the surface of the hemisphere.*

XIII. Let S' denote the spherical base of the supplemental cone, and L the lateral surface of the given cone contained within the sphere, then from the preceding equations we have

$$\mathrm{S} + 2\,\mathrm{L}' = 2\,c^2\pi, \quad\quad \mathrm{S}' + 2\,\mathrm{L} = 2\,c^2\pi; \quad . \quad . \quad (28.)$$

adding these equations,

$$(\mathrm{S} + 2\,\mathrm{L}) + (\mathrm{S}' + 2\,\mathrm{L}') = 4\,c^2\pi; \quad . \quad . \quad . \quad (29.)$$

subtracting $\mathrm{S} - \mathrm{S}' = 2\,(\mathrm{L} - \mathrm{L}'); \quad . \quad . \quad . \quad (30.)$

or,

XIV. *If any two concentric cones, supplemental to each other, be cut by a concentric sphere, the sum of their spherical bases, together with twice their lateral surfaces, is equal to the surface of the sphere.*

And the difference of their bases is equal to twice the difference of their lateral surfaces. Hence also this other theorem:

[*] The discovery of this remarkable relation between the length and area of a spherical ellipse is due to Professor MacCullagh, to whom mathematical science is so much indebted for many new and beautiful theorems in this department of geometrical research.

XV. *Let a cone whose principal angles are supplemental be cut by a concentric sphere, the sum of the two spherical bases, together with twice the lateral surface comprised within the sphere, is equal to the surface of the sphere.*

XVI. We shall now proceed to establish some other analogies between plane and spherical ellipses.

To investigate the formula $s = \int p \, d\lambda \pm u$ for the rectification of a plane curve, where p is the perpendicular from any assumed point called the pole on a tangent to the curve, λ the angle between this perpendicular and any fixed line drawn through the pole, u the portion of the tangent intercepted between the point of contact and the foot of this perpendicular.

Let Q be the centre of curvature of the arc at A, A B a tangent at A, O B a perpendicular from the pole O upon the tangent, O C a perpendicular upon the radius of curvature Q A, then A B = O C; assume a point a indefinitely near to A; let ab be a tangent at a, O b a perpendicular from O upon this tangent, O T a perpendicular from O upon the radius of curvature Q a; let O C cut the radius Q a in t, and through t let $t \, n$ be

Fig. 3.

drawn parallel to the radius Q A, then A a = differential of the arc $= d s = $ A $n + n a$; now A $n =$ C $t =$ O T $-$ O C $= a b$ $-$ A B $= d u$, and $n a = n t \times \angle$ at $n = p \, d\lambda$; hence

$$\frac{ds}{d\lambda} = p + \frac{du}{d\lambda} \cdot \quad . \quad . \quad . \quad . \quad . \quad (31.)$$

It will be seen that in the proof of this theorem the radius of curvature is assumed as being greater than p; should it, on the contrary, be less, the expression becomes in that case

$$\frac{ds}{d\lambda} = p - \frac{du}{d\lambda};$$

hence generally $s = \int p \, d\lambda \pm u.$.   .   .   (31*.) *bis.*

It is obvious also that when the perpendicular p is equal to the radius of curvature, at that point of the curve $\dfrac{du}{d\lambda} = 0$, or u is there a maximum or a minimum.
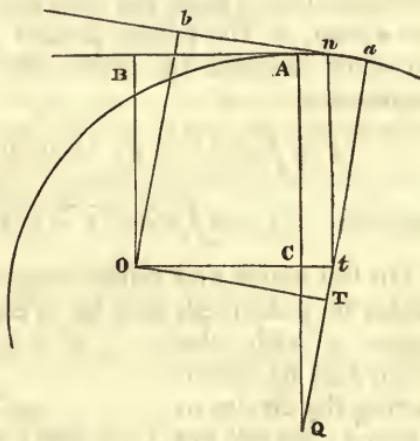
From these considerations it easily follows that u, the portion of the tangent between the point of contact and the foot of the perpendicular, is either a maximum or minimum when the radius of curvature is equal to p; for then $\dfrac{d u}{d \lambda} = 0$.

It is also manifest that $\qquad \dfrac{d p}{d \lambda} = u; \quad . \quad . \quad . \quad . \quad . \quad$ (32.)

for $\qquad t\,\mathrm{T} = \mathrm{T}\,a - t\,a = \mathrm{T}\,a - c\,\mathrm{A} = p' - p = d p,$

and $\quad \mathrm{O\,T} = u + d u,$ but $\mathrm{O\,T} \times \angle t\,\mathrm{O\,T} = t\,\mathrm{T},\ t\,\mathrm{O\,T} = d\lambda;$

hence $\qquad u = \dfrac{d p}{d \lambda}$, since $d u\ d\lambda$ is of the second order.

XVII. To apply this formula to the rectification of the ellipse, let the centre be the pole, λ the angle between the perpendicular p from the centre on the tangent, and the major-axe; then, as the perpendicular is greater than the radius of curvature towards the vertex of the curve which lies on the major-axe,

$$s = \int p\, d \lambda - u; \quad \text{now } p = a \sqrt{1 - e^2 \sin^2 \lambda};$$

hence $\qquad s = a \int d\lambda \sqrt{1 - e^2 \sin^2 \lambda} - u. \quad . \quad . \quad$ (33.)

On the major and minor axes of the ellipse as diameters let circles be described, and let a common diameter making the angle φ with the minor axis be drawn cutting the circles in m and n; let fall the ordinates ms and nt, then $ms = x = a \sin \varphi$, and $nt = y = b \cos \varphi$. Now these, it is easy to show, are the co-ordinates of the ex-

Fig. 4.

tremity of the arc $\mathrm{B\,Q}$ of the ellipse measured from the vertex of the minor axis. Differentiating x and y, φ being the independent variable, and substituting the resulting values in the common formula for rectification $\dfrac{d s}{d x} = \sqrt{1 + \dfrac{d y^2}{d x^2}}$, we find

$$s' = a \int d \varphi \sqrt{1 - e^2 \sin^2 \varphi}. \quad . \quad . \quad . \quad (34.)$$

If now the integrals in (33.) and (34.) be taken between the same limits for λ and φ, the values of the expressions under the sign of integration will be equivalent, equal to K suppose; hence $\qquad s = \mathrm{K} - u,\ s' = \mathrm{K};$
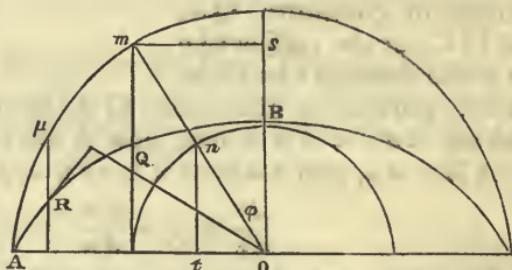
therefore $\qquad s' - s = u. \quad . \quad . \quad . \quad . \quad . \quad . \quad$ (35.)

Hence we may take on an elliptic quadrant two arcs measured from the extremities of the minor and major axes respectively, whose difference shall be equal to a right line.

XVIII. It is not difficult to show that the extremities of these arcs are the points of intersection of the given ellipse with two hyperbolas having the same *foci* as the given ellipse, one passing through the extremity of the arc measured from the minor axe, whose axes A, B, are given by the equations

$$ A = a\,e \sin \lambda, \quad B = a\,e \cos \lambda; \quad . \quad . \quad . \quad (36.) $$

the other passing through the extremity of the arc measured from the major axe, its semiaxes A′, B′ being deduced from the equations

$$ A' = \frac{a\,e \cos \lambda}{\sqrt{1 - e^2 \sin^2 \lambda}}, \quad B' = \frac{b\,e \sin \lambda}{\sqrt{1 - e^2 \sin^2 \lambda}}. \quad . \quad . \quad (37.) $$

XIX. To determine the general value of u,

as $\qquad u = \dfrac{dp}{d\lambda} = \dfrac{d.}{d\lambda}\, a \sqrt{1 - e^2 \sin^2 \lambda}, \quad u = \dfrac{a\,e^2 \sin \lambda \cos \lambda}{\sqrt{1 - e^2 \sin^2 \lambda}}.$

We may hence deduce some remarkable relations between u, a, b, A, B, A′, B′; for by the help of the preceding equations it is easily shown that

$$ a\,u = A\,A', \quad b\,u = B\,B', \quad \frac{B\,B'}{A\,A'} = \frac{b}{a}. \quad . \quad . \quad (38.) $$

Let 2θ and $2\theta'$ be the angles between the asymptots of those hyperbolas, then

$$ \tan \theta = \frac{B}{A} = \cot \lambda, \quad \text{and} \quad \tan \theta' = \frac{b}{a} \tan \lambda; \quad . \quad (39.) $$

hence $\qquad \tan \theta \tan \theta' = \dfrac{b}{a},$

a result independent of λ.

XX. Let r' and r'' be the semidiameters of the ellipse measured along these asymptots, then

$$ \frac{\cos^2 \theta}{a^2} + \frac{\sin^2 \theta}{b^2} = \frac{1}{r'^2}; $$

or putting for $\cos \theta$, $\sin \theta$ their values deduced from (39.), we find

$$ r'^2 = \frac{a^2 b^2}{a^2 \cos^2 \lambda + b^2 \sin^2 \lambda} = \frac{a^2 b^2}{p^2}. $$

In like manner it may be shown that

$$ r''^2 = a^2 \cos^2 \lambda + b^2 \sin^2 \lambda = p^2; $$

hence $\qquad r'\, r'' = a\,b, \quad . \quad . \quad . \quad . \quad . \quad$ (40.)

a result also independent of λ.

We have thus the remarkable result that the segments of

the asymptots between the centre and the curve are, the one a fourth proportional to the perpendicular and the semiaxes, while the other is equal to the perpendicular itself.

XXI. To find when the difference of the elliptic arcs is a maximum; in this case u is a maximum, or $\dfrac{d u}{d \lambda} = 0$, but $u = \dfrac{d p}{d \lambda}$; hence

$$\frac{d^2 p}{d \lambda^2} = 0, \text{ or } \frac{d^2 .}{d \lambda^2} a \sqrt{1 - e^2 \sin^2 \lambda} = 0.$$

From this equation we find

$$\tan^2 \lambda = \frac{a}{b}. \quad . \quad . \quad . \quad . \quad . \quad (41.)$$

Deducing from this value of $\tan \lambda$ the values of $\sin \lambda$, $\cos \lambda$, and substituting in (36.) and (37.), we find

$$\text{A} = a\,(a-b), \; \text{B} = b\,(a-b), \; \text{A}' = a\,(a-b), \; \text{B} = b\,(a-b), \; . \; (42.)$$

or
$$\text{A} = \text{A}', \; \text{B} = \text{B}'.$$

In this case, then, when the difference of the elliptic arcs is a maximum, the two confocal hyperbolas become identical, and therefore the two elliptic arcs constitute the quadrant; this is the well-known theorem of Fagnani.

To find the corresponding value of u, as

$$a u = \text{A A}' = a\,(a - b), \; u = a - b; \quad . \quad . \quad (43.)$$

also as
$$r' = r'', \text{ and } r'' = p, \; p^2 = a\,b; \quad . \quad . \quad (44.)$$

hence the semidiameter of the ellipse along the asymptot is equal to the perpendicular from the centre. In this case the whole quadrant is divided into two arcs whose difference is equal to the difference of the semiaxes, and this point may, for the sake of distinction, be called the point of *linear* section.

The locus of this point for a series of confocal ellipses may be shown to be the curve whose equation is

$$a^2 e^2 = (x^{\frac{2}{3}} - y^{\frac{2}{3}})\,(x^{\frac{2}{3}} + y^{\frac{2}{3}})^2.$$

Let tangents be drawn to the ellipse at the point of *linear* section and produced to meet the adjacent axes; calling the segment of the tangent terminated in the minor axe t, the other terminated in the major axe t', it can be easily shown that

$$t = \tan \lambda \sqrt{a^2 \cos^2 \lambda + b^2 \sin^2 \lambda},$$

$$t' = \frac{b^2 \tan \lambda}{\sqrt{a^2 \cos^2 \lambda + b^2 \sin^2 \lambda}};$$

hence $\quad t - t' = \dfrac{a\,e^2 \sin \lambda \cos \lambda}{\sqrt{a^2 \cos^2 \lambda + b^2 \sin^2 \lambda}} = u.. \quad . \quad (44^*.)\,bis.$

XXII. It would be easy to show, were it not too wide a

digression from the main subject of this paper, that if a series of concentric ellipses be described having coincident axes, and the sum of their semiaxes constant equal to L suppose, the locus of their points of *linear section* will be a hypocycloid, concentric with the ellipses; the radius of whose generating circle $= L$, and the radius of whose rolling circle is $= \frac{1}{4} L$; and also that the difference between the elliptic arcs is to the difference between the corresponding hypocycloidal arcs in the constant ratio of $2 : 3$.

XXIII. A formula analogous to (31.) may be established for the rectification of any curve on the surface of a sphere formed by the intersection of this surface with a concentric cone of any order.

In the first place, let the cone be of the second degree, and let a plane be drawn perpendicular to the axis of this cone, touching the sphere and cutting the cone in the *elliptic base*; let a tangent plane (T) be drawn to the cone, cutting the plane of the elliptic base in a right line u, a tangent to this ellipse, and the surface of the sphere in an arc of a great circle, touching the spherical ellipse; let the distance from the centre of the sphere to the point of contact of the tangent with the ellipse be R; through the centre of the sphere let a plane (H) be drawn perpendicular to u, then as u is a right line as well in the plane (T) as in the *elliptic base*, the plane (H) is perpendicular both to the tangent plane (T) and to the base of the cone; hence the plane (H) passes through the axis of the cone and the centre of the plane ellipse, as also of the spherical ellipse, cutting the former in a perpendicular p from the centre on the tangent u, and the latter in an arc ϖ of a great circle, perpendicular to the tangent arc to the spherical ellipse; for the two latter arcs must be at right angles to each other, since the planes (T) and (H) are at right angles. Let P be the distance from the centre of the sphere to the point where the plane (H) cuts the right line u, r the distance from the centre of the plane ellipse to the point of contact of u with it; then to any one attending to this construction it will be manifest that (c being the radius of the sphere)

$$R^2 = c^2 + r^2, \quad P^2 = c^2 + p^2, \quad R^2 = P^2 + u^2. \quad . \quad (45.)$$

XXIV. Let $d s$ be the element of an arc of the ellipse between any two consecutive values of R indefinitely near to each other, $c\, d\sigma$ the corresponding element of the spherical ellipse between the same consecutive positions of R; then the areas of the elementary triangles on the surface of the cone between these consecutive positions of R having their vertices at the centre of the sphere and their bases an element of the arc of the ellipse and of an arc of the spherical ellipse respectively,

are as their bases multiplied by their altitudes; calling these areas M and N, we have

$$M : N :: ds \times P : c\, d\sigma \times c;$$

but these areas are manifestly as the squares of the sides of the elementary triangles, or

$$M : N :: R^2 : c^2,$$

Hence

$$d\sigma = \frac{P\, ds}{R^2}, \quad \cdots \quad \cdots \quad (46.)$$

an expression for the element of an arc, the intersection of a concentric cone with a spherical surface whose radius is 1.

Substituting in the formula $\dfrac{ds}{d\lambda} = p + \dfrac{du}{d\lambda}$ (31.) the value of $\dfrac{ds}{d\lambda}$ in terms of $\dfrac{d\sigma}{d\lambda}$, we find

$$\frac{d\sigma}{d\lambda} = \frac{P\,p}{R^2} + \frac{P}{R^2}\frac{du}{d\lambda}.$$

Now ϖ being the arc which p subtends at the centre of the sphere, $p = P \sin \varpi$ and $P^2 = R^2 - u^2$, making these substitutions in the last formula, the resulting equation becomes

$$\frac{d\sigma}{d\lambda} = \sin \varpi + \frac{1}{R^2} \left\{ P\frac{du}{d\lambda} - u^2 \sin \varpi \right\}.$$

Now

$$\sin \varpi = \frac{p}{P},\ u = \frac{dp}{d\lambda},\ \frac{du}{d\lambda} = \frac{d^2 p}{d\lambda^2} \text{ and } \frac{P\, dP}{d\lambda} = p\frac{dp}{d\lambda};\ \cdot\ (47.)$$

making these substitutions in the preceding equation,

$$\frac{d\sigma}{d\lambda} = \sin \varpi + \frac{1}{R^2} \left\{ P\frac{d^2 p}{d\lambda^2} - \frac{dP}{d\lambda}\frac{dp}{d\lambda} \right\}. \quad \cdots \quad (48.)$$

XXV. We now proceed to show that the last term of this equation is the differential of the arc with respect to λ, subtended by u at the centre of the sphere.

Let this arc be v, then $\tan v = \dfrac{u}{P}$, $\cos v = \dfrac{P}{R}$; differentiating the first of these equations and eliminating $\cos v$ by the aid of the second,

$$\frac{dv}{d\lambda} = \frac{1}{R^2} \left\{ P\frac{du}{d\lambda} - u\frac{dP}{d\lambda} \right\}, \text{ but } \frac{du}{d\lambda} = \frac{d^2 p}{d\lambda^2}, u = \frac{dp}{d\lambda};$$

hence

$$\frac{dv}{d\lambda} = \frac{1}{R^2} \left\{ P\frac{d^2 p}{d\lambda^2} - \frac{dP}{d\lambda}\frac{dp}{d\lambda} \right\}. \quad \cdots \quad (49.)$$

Subtracting (46.) from (45.), we find

$$\frac{d\sigma}{d\lambda} = \sin \varpi + \frac{dv}{d\lambda}, \text{ or } \sigma = \int d\lambda \left[\sin \varpi\right] + v, \quad \cdot \quad (50.)$$

a formula for the rectification of curves on the surface of a sphere analogous to (31.).

As in none of the successive steps of the preceding demonstration is any reference made to the peculiar properties of curves or cones of the second degree, it is clear that the preceding formula will hold for the rectification of any curve upon the surface of a sphere, the intersection of this surface with a concentric cone of any order, and as a curve traced *liberâ manu* on the surface of a sphere may be constituted the base of a cone whose vertex shall be at the centre of the sphere, it is plain that the above formula may be applied to the rectification of any curve upon the surface of a sphere.

Hence as an arc of any plane curve may be expressed by means of a definite integral and a finite right line, so may the arc of any curve described on the surface of a sphere be exhibited by means of a definite integral and an arc of a circle.

XXVI. *To apply this formula to the rectification of the spherical ellipse.*

Let a and b be the semiaxes of the elliptic base, r the central radius vector drawn to the point of contact of the tangent u, p the perpendicular from the centre on the tangent, u the intercept of this tangent between the point of contact and the foot of the perpendicular; let α, β, ρ, ϖ, v be the angles subtended at the centre of the sphere whose radius is c by the lines a, b, r, p, u, then

$a = c \tan \alpha,\ b = c \tan \beta,\ r = c \tan \rho,\ p = c \tan \varpi$ and $u = \mathrm{P} \tan v$.

Now in the plane ellipse $p^2 = a^2 \cos^2 \lambda + b^2 \sin^2 \lambda$; hence

$$\tan^2 \varpi = \tan^2 \alpha \cos^2 \lambda + \tan^2 \beta \sin^2 \lambda,$$
and
$$1 = \cos^2 \lambda + \sin^2 \lambda.$$

Adding these equations together,

$$\sec^2 \varpi = \sec^2 \alpha \cos^2 \lambda + \sec^2 \beta \sin^2 \lambda;$$

dividing the former equation by the latter,

$$\sin^2 \varpi = \frac{\tan^2 \alpha \cos^2 \lambda + \tan^2 \beta \sin^2 \lambda}{\sec^2 \alpha \cos^2 \lambda + \sec^2 \beta \sin^2 \lambda}. \quad \cdot \quad \cdot \quad (51.)$$

Substituting this value of $\sin \varpi$ in (47.), we obtain the equation

$$\sigma = \int d\lambda \sqrt{\frac{\tan^2 \alpha \cos^2 \lambda + \tan^2 \beta \sin^2 \lambda}{\sec^2 \alpha \cos^2 \lambda + \sec^2 \beta \sin^2 \lambda}} + v. \quad \cdot \quad \cdot \quad (52.)$$

XXVII. To investigate *another formula for rectification.*

Assume $\quad \sin^2 \rho = \sin^2 \alpha \sin^2 \varphi + \sin^2 \beta \cos^2 \varphi;\quad \cdot \quad \cdot \quad (53.)$
hence $\quad \cos^2 \rho = \cos^2 \alpha \sin^2 \varphi + \cos^2 \beta \cos^2 \varphi.$
Substituting these values in (18.), we find

$$\sigma' = \int d\varphi \sqrt{\frac{\tan^2 \alpha \cos^2 \varphi + \tan^2 \beta \sin^2 \varphi}{\sec^2 \alpha \cos^2 \varphi + \sec^2 \beta \sin \varphi}}. \quad \cdot \quad \cdot \quad (54.)$$

Now if the integrals in the last two equations be taken between the same limits of λ and φ, their values will be equal, hence, subtracting the former from the latter,

$$\sigma' - \sigma = - \upsilon.$$

Now as $\sin \upsilon = \dfrac{u}{P}$ and $u = \dfrac{dp}{d\lambda}$, $\sin \upsilon = \dfrac{p\dfrac{dp}{d\lambda}}{Pp}$, and as neither

P nor p pass through infinity or zero, they always retain the same sign $+$, hence the sign of $\sin \upsilon$ will depend upon that of $\dfrac{dp}{d\lambda}$, but $p^2 = a^2 \cos^2\lambda + b^2 \sin^2\lambda$; hence

$$p\frac{dp}{d\lambda} = - (a^2 - b^2) \sin \lambda \cos \lambda,$$

therefore $\sin \upsilon$ is negative, and as υ is always less than π, υ is negative, and may be written $- \upsilon$; making this change in the last formula for rectification,

$$\sigma' - \sigma = \upsilon, \quad . \quad . \quad . \quad . \quad . \quad . \quad (55.)$$

a formula precisely analogous to (35.).

Thus as the difference of two elliptic arcs may be exhibited by a right line, so may the difference of two arcs of a spherical ellipse be represented by an arc of a great circle.

XXVIII. To show the geometrical interpretation of the assumption made in (53.).

In the first place we may observe, that if O A, O B are arcs of great circles at right angles, a point P on the surface of the sphere may be referred to those axes either by the arcs P m, P n, which are secondaries to the arcs O A, O B, or by the arcs O m, O n; let P $m = \breve{y}$, P $n = \breve{x}$, O $m = \xi$, O $n = \eta$, O P $= \rho$, and the angle P O $m = \omega$; we shall then have by the common rules for right-angled spherical triangles,

Fig. 5.

$$\left.\begin{array}{ll} \sin \breve{y} = \sin \rho \sin \omega, & \sin \breve{x} = \sin \rho \cos \omega \\ \tan \eta = \tan \rho \sin \omega, & \tan \xi = \tan \rho \cos \omega \end{array}\right\} \quad . \quad . \quad (56.)$$

hence $\quad \sin^2 \breve{y} + \sin^2 \breve{x} = \sin^2\rho, \quad \tan^2\xi + \tan^2\eta = \tan^2\rho.$

We may easily establish a relation between $\breve{x}$ and ξ, $\breve{y}$ and η, for in the preceding equations, eliminating the functions of ω, we find

$$\sin \breve{x} = \tan \xi \cos \rho, \quad \sin \breve{y} = \tan \eta \cos \rho;$$

by the help of these equations we may pass from the one system of spherical coordinates to the other.

If now, between equations (4.), (5.) and (56.), we eliminate
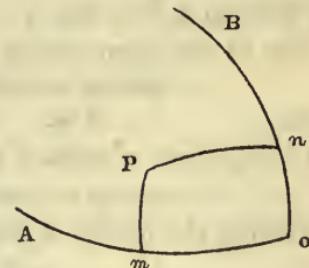
$\sin \omega$, $\cos \omega$ successively, we shall obtain for the equations of the spherical ellipse,

$$\frac{\sin^2 \breve{x}}{\sin^2 \alpha} + \frac{\sin^2 \breve{y}}{\sin^2 \beta} = 1, \quad \frac{\tan^2 \xi}{\tan^2 \alpha} + \frac{\tan^2 \eta}{\tan^2 \beta} = 1. \quad . \quad (57.)$$

On the major and minor axes of the spherical ellipse as diameters, let circles be described (see the fig. page 26), and let a great circle be drawn through the centre of the ellipse, making the angle ϕ with the minor axis and meeting the circles in the points m and n; through m and n let arcs of great circles $m\,s$ and $n\,t$ be drawn at right angles to the spherical axes O B, O A; then as O$m\,s$ is a right-angled spherical triangle, $\sin m\,s = \sin \alpha \sin \phi$, in like manner $\sin n\,t = \sin \beta \cos \phi$, or $\dfrac{\sin^2 m\,s}{\sin^2 \alpha} + \dfrac{\sin^2 n\,t}{\sin^2 \beta} = 1$; it follows then that $m\,s$ and $n\,t$ are the coordinates of a point on the spherical ellipse.

Let ρ' be the central radius vector of this point, then $\sin^2 \rho' = \sin^2 m\,s + \sin^2 n\,t = \sin^2 \alpha \sin^2 \phi + \sin^2 \beta \cos^2 \phi$, but in (53.) $\sin^2 \rho = \sin^2 \alpha \sin^2 \phi + \sin^2 \beta \cos^2 \phi$, hence $\rho = \rho'$, or in (53.) ρ is the central radius vector of a point of which the coordinates are $\sin \alpha \sin \phi$, and $\sin \beta \cos \phi$ respectively.

XXIX. To find when v is a maximum.

In this case $\dfrac{dv}{d\lambda} = 0$, or from (49.) $\dfrac{dp}{d\lambda}\dfrac{d\mathrm{P}}{d\lambda} = \mathrm{P}\dfrac{d^2 p}{d\lambda^2}$. . (58.)

Now $p = \sqrt{a^2 \cos^2 \lambda + b^2 \sin^2 \lambda}$, $\mathrm{P} = \sqrt{c^2 + a^2 \cos^2 \lambda + b^2 \sin^2 \lambda}$;

hence $\dfrac{dp}{d\lambda} = \dfrac{-(a^2 - b^2) \sin \lambda \cos \lambda}{\sqrt{a^2 \cos^2 \lambda + b^2 \sin^2 \lambda}}$,

$\dfrac{d\mathrm{P}}{d\lambda} = \dfrac{-(a^2 - b^2) \sin \lambda \cos \lambda}{\sqrt{c^2 + a^2 \cos^2 \lambda + b^2 \sin^2 \lambda}}$,

$\dfrac{d^2 p}{d\lambda^2} = \dfrac{-(a^2 - b^2)(a^2 \cos^4 \lambda - b^2 \sin^4 \lambda)}{\{a^2 \cos^2 \lambda + b^2 \sin^2 \lambda\}^{\frac{3}{2}}}$.

Making these substitutions in (58.) and putting $\tan \alpha$ for $\dfrac{a}{c}$, $\tan \beta$ for $\dfrac{b}{c}$, we find

$$\tan^2 \lambda = \frac{\tan \alpha \sec \alpha}{\tan \beta \sec \beta} = \frac{\sin \alpha}{\sin \beta} \sec^2 \varepsilon, \quad . \quad . \quad (59.)$$

a result analogous to (41.).

XXX. To find a general expression for the value of v; as

$$\tan^2 v = \frac{u^2}{\mathrm{P}^2} = \frac{u^2 p^2}{\mathrm{P}^2 p^2} = \frac{(a^2 - b^2)^2 \sin \lambda \cos \lambda}{(a^2 \cos^2 \lambda + b^2 \sin^2 \lambda)(c^2 + a^2 \cos^2 \lambda + b^2 \sin^2 \lambda)},$$

we shall have, introducing the relations $\tan \alpha = \dfrac{a}{c}$, $\tan \beta = \dfrac{b}{c}$,

and those given in (11.) and (12.),

$$\tan \upsilon = \frac{e^2 \sin \alpha \sin \lambda \cos \lambda}{\sqrt{1 - e^2 \sin^2 \lambda} \sqrt{1 - \sin^2 \varepsilon \sin^2 \lambda}}. \quad \cdot \quad \cdot \quad (60.)$$

XXXI. Having already exceeded the conventional limits of a mathematical paper in this Journal, it may suffice to give the enunciations of a few theorems on the spherical ellipse analogous to those which have been already established on the plane ellipse, postponing their discussion to a future occasion.

Through the extremities of the arcs of the spherical ellipse two spherical hyperbolas may be drawn having the same focus as the spherical ellipse; calling the axes of the one nearer to the minor axe 2 A and 2 B, the axes of the other passing through the extremity of the arc measured from the major axe 2 A′ and 2 B′, we may with little difficulty establish the following relations:—

$$\tan^2 A = \frac{\sin^2 \varepsilon \sin^2 \lambda}{1 - \sin^2 \varepsilon \sin^2 \lambda}, \quad \tan^2 B = \frac{\sin^2 \varepsilon \cos^2 \lambda}{1 - \sin^2 \varepsilon \sin^2 \lambda}, \quad \cdot \quad (61.)$$

$$\tan^2 A' = \frac{\tan^2 \varepsilon \cos^2 \lambda}{1 - e^2 \sin^2 \lambda,} \quad \tan^2 B' = \frac{e^2 \cos^2 \varepsilon \sin^2 \beta \sin^2 \lambda}{1 - e^2 \sin^2 \lambda}. \quad (62.)$$

We may hence show that

$$\left.\begin{array}{l} \tan \upsilon \tan \beta \cos \alpha = \tan B \tan B' \\ \tan \upsilon \tan \alpha \cos \beta = \tan A \tan A' \end{array}\right\}, \quad \cdot \quad \cdot \quad (63.)$$

$$\frac{\tan B \tan B'}{\tan A \tan A'} = \frac{\tan \beta \sec \beta}{\tan \alpha \sec \alpha},$$

results analogous to (38.).

In the spherical hyperbola ε' being the eccentricity, we shall find

$$\tan^2 \varepsilon' = \frac{\tan^2 A + \tan^2 B}{1 - \tan^2 B},$$

A and B being the semiaxes, while ε being the eccentricity of the spherical ellipse whose semiaxes are α and β,

$$\tan^2 \varepsilon = \frac{\tan^2 \alpha - \tan^2 \beta}{1 + \tan^2 \beta}.$$

Let ε'' be the eccentricity of the hyperbola whose semiaxes are A′ and B′, we shall find, putting for A, B, A′, B′, their values given above,

$$\varepsilon = \varepsilon' = \varepsilon''.$$

When υ is a maximum we have found for the corresponding value of $\tan^2 \lambda$ the expression

$$\frac{\sin \alpha}{\sin \beta} \sec^2 \varepsilon.$$

Substituting the values of $\sin \lambda$, $\cos \lambda$, thence derived in (61.) and (62.), there results

$$\left.\begin{aligned}
\tan^2 A &= \tan \alpha \sec \alpha \, (\sin \alpha - \sin \beta)\\
\tan^2 B &= \tan \beta \sec \beta \, (\sin \alpha - \sin \beta)\\
\tan^2 A' &= \tan \alpha \sec \alpha \, (\sin \alpha - \sin \beta)\\
\tan^2 B' &= \tan \beta \sec \beta \, (\sin \alpha - \sin \beta)
\end{aligned}\right\} ; \quad \cdot \quad \cdot \quad (64.)$$

hence $A = A'$, $B = B'$; or when v is a maximum the two hyperbolas coalesce, and the arcs of the ellipse have a common extremity, or constitute the quadrant, and this point may be termed the point of *circular section*.

XXXII. To find the value of v when v is a maximum; as
$\tan v \tan \alpha \cos \beta = \tan A \tan A' = \tan^2 A = \tan \alpha \sec \alpha (\sin \alpha - \sin \beta)$

$$\tan v = \sec \alpha \sec \beta \, (\sin \alpha - \sin \beta). \quad . \quad . \quad . \quad (65.)$$

XXXIII. To find the values of the arcs of the asymptotic circles to the hyperbolas contained within the spherical ellipse.

The asymptotic circles to the spherical hyperbola are the great circles whose planes are parallel to the circular sections of the cone, of which these hyperbolas are sections.

Let 2θ be the angle between the great circles which constitute the asymptots of the hyperbola passing through the extremity of the arc measured from the minor axe; then, as the asymptotic circles are parallel to the circular sections of the cone, and whose principal semiangles are α' and β', of which the given hyperbola is a section, we shall have (see (21.)),

$$\sin^2 \theta = \frac{\sin^2 \beta'}{\sin^2 \alpha'};$$

but it may be shown that

$$\sin \beta' = \cos \lambda \text{ and } \sin \alpha' = \cos A;$$

hence $\qquad \sin^2 \theta = \dfrac{\cos^2 \lambda}{\cos^2 A}.$

Substituting for $\cos^2 A$ its value derived from (61.), we find

$$\tan \theta = \frac{\cos \beta}{\cos \alpha} \cot \lambda. \quad . \quad . \quad . \quad (66.)$$

Eliminating θ between this equation and the equation of the ellipse $\dfrac{\cos^2 \theta}{\sin^2 \alpha} + \dfrac{\sin^2 \theta}{\sin^2 \beta} = \dfrac{1}{\sin^2 \rho}$, there results

$$\frac{\tan^2 \alpha \cos^2 \lambda + \tan^2 \beta \sin^2 \lambda}{\sec^2 \alpha \cos^2 \lambda + \sec^2 \beta \sin^2 \lambda} = \frac{\sin^2 \alpha \sin^2 \beta}{\sin^2 \rho}; \quad . \quad (67.)$$

but it has been shown (51.) that the first member of this equation $= \sin^2 \varpi$.

Making this substitution,

$$\sin \rho = \frac{\sin \alpha \sin \beta}{\sin \varpi}. \quad . \quad . \quad . \quad . \quad (68.)$$

D 2

XXXIV. Let $2\,\theta'$ be the angle between the asymptots of the spherical hyperbola passing through the extremity of the arc measured from the major axe, then, as before, $\sin\theta' = \dfrac{\sin\beta''}{\sin\alpha''}$, α'' and β'' being the *principal* semiangles of the cone of which this hyperbola is a section.

It may with little trouble be proved that

$$\tan\beta'' = \frac{\tan B'}{\tan A'}, \qquad \sin\alpha'' = \cos A'.$$

Substituting for the functions of A' and B' in these equations their values given in (62.), we find, after some obvious reductions,

$$\sin^2\theta' = \frac{\tan^2\beta\sin^2\lambda}{\tan^2\alpha\cos^2\lambda + \tan^2\beta\sin^2\lambda},$$

$$\cos^2\theta' = \frac{\tan^2\alpha\cos^2\lambda}{\tan^2\alpha\cos^2\lambda + \tan^2\beta\sin^2\lambda};$$

hence $\qquad \tan\theta' = \dfrac{\tan\beta}{\tan\alpha}\tan\lambda.$ . . . . (69.)

Multiplying (66.) (69.) together, we obtain

$$\tan\theta\tan\theta' = \frac{\sin\beta}{\sin\alpha}, \quad . \quad . \quad . \quad . \quad . \quad . \quad (70.)$$

a result independent of λ, and in strict conformity with (39.).

In the polar equation of the ellipse, substituting the values of $\sin\theta'\cos\theta'$ given above, ρ' being the corresponding radius vector, we obtain the resulting equation

$$\sin^2\rho' = \frac{\tan^2\alpha\cos^2\lambda + \tan^2\beta\sin^2\lambda}{\sec^2\alpha\cos^2\lambda + \sec^2\beta\sin^2\lambda}; \quad . \quad . \quad (71.)$$

hence $\qquad\qquad \sin\rho\sin\rho' = \sin\alpha\sin\beta.$ . . . . . (72.)

a result also independent of λ.

We have thus $\rho' = \varpi$; or the semidiameter of the ellipse along the asymptot of this hyperbola is equal to the perpendicular from the centre on the tangent to the ellipse drawn through the point R of intersection of the ellipse and hyperbola. See fig. 4.

XXXV. Let $\breve{r}$ and $\breve{r}'$ be the semidiameters of the spherical ellipse passing through the points m, μ, in which the ordinates of the extremities of the elliptic arcs being produced meet the circle on the major axe; let ϑ and ϑ' be the angles which $\breve{r}$ and $\breve{r}'$ make with the major axe (fig. 4), then $\vartheta = \dfrac{\pi}{2} - \lambda$, and the value of ϑ' may be thus found; calling H the spherical coordinate of the point μ on the circle,

$$\frac{\tan^2\xi}{\tan^2\alpha} + \frac{\tan^2 H}{\tan^2\alpha} = 1 \text{ in the circle,}$$

and $\qquad \dfrac{\tan^2 \xi}{\tan^2 \alpha} + \dfrac{\tan^2 \eta}{\tan^2 \beta} = 1$ in the ellipse;

hence $\qquad \dfrac{\tan H}{\tan \alpha} = \dfrac{\tan \eta}{\tan \beta};$

and by the rules for right-angled triangles,

$$\frac{\tan^2 H}{\tan^2 \alpha} = \sin^2 \vartheta',$$

or $\qquad \sin^2 \vartheta' = \dfrac{\tan^2 \eta}{\tan^2 \beta} = \dfrac{\tan^2 \beta \sin^2 \lambda}{\tan^2 \alpha \cos^2 \lambda + \tan^2 \beta \sin^2 \lambda}.$

Eliminating ϑ' between this equation and the polar equation of the spherical ellipse,

$$\tan^2 \overset{\smile}{r}' = \tan^2 \alpha \cos^2 \lambda + \tan^2 \beta \sin^2 \lambda; \quad . \quad . \quad (73.)$$

and as $\vartheta = \dfrac{\pi}{2} - \lambda,$

$$\tan^2 \overset{\smile}{r} = \frac{\tan^2 \alpha \tan^2 \beta}{\tan^2 \alpha \cos^2 \lambda + \tan^2 \beta \sin^2 \lambda},$$

or $\qquad\qquad \tan \overset{\smile}{r} \tan \overset{\smile}{r}' = \tan \alpha \tan \beta. \quad . \quad . \quad . \quad (74.)$

XXXVI. Resuming the values of the angles which the asymptots of the spherical hyperbolas, as also the diameters of the ellipse through the points m and μ on the circle make with the major axe, we find

$$\left. \begin{array}{ll} \tan \theta = \dfrac{\cos \beta}{\cos \alpha} \cot \lambda, & \tan \vartheta = \cot \lambda \\[2mm] \tan \theta' = \dfrac{\tan \beta}{\tan \alpha} \tan \lambda, & \tan \vartheta' = \dfrac{\tan \beta}{\tan \alpha} \tan \lambda \end{array} \right\} \quad . \quad . \quad (75.)$$

We may here perceive a remarkable interruption of the analogy which has been found hitherto to exist between the properties of plane and spherical conics, while in the plane section the asymptots to the confocal hyperbolas coincide with the diameters drawn through the points $m \, \mu$, as is also true of the spherical hyperbola adjacent to the major axe; the asymptot of the hyperbola nearer the minor axe does not coincide with the diameter through the point m; in other words, while $\theta' = \vartheta'$, θ is not equal to ϑ.

XXXVII. Two tangents being drawn to the spherical ellipse at the point of *circular section* and produced to meet the adjacent axes, to find the values of those circular arcs.

Let η be the coordinate of this point along the axis of Y, and z the point in which the tangent arc τ cuts the minor axis; let $oz = \zeta$, then $\tan \eta \tan \zeta = \tan^2 \beta$; and as τ, $\overset{\smile}{x}$ and $(\zeta - \eta)$ are the sides of a right-angled spherical triangle,

$\cos \tau = \cos \overset{\smile}{x} \cos (\zeta - \eta) = \cos \overset{\smile}{x} \cos \zeta \cos \eta + \cos \overset{\smile}{x} \sin \zeta \sin \eta.$

Now as $\tan \zeta = \dfrac{\tan^2 \beta}{\tan \eta}$, we may, eliminating ζ from the last equation, find

$$\cos \tau = \frac{\cos \breve{x} \sin \eta \sec^2 \beta}{\sqrt{\tan^4 \beta + \tan^2 \eta}},$$

and η being the common ordinate of the ellipse and hyperbola,

$$\tan^2 \eta = \frac{\sin^2 \beta \cos^2 \lambda}{\cos^2 \alpha \sin^2 \lambda + \cos^2 \beta \cos^2 \lambda} :$$

we have also $\qquad \sin \breve{x} = \sin \alpha \sin \lambda.$

Making the necessary substitutions deduced from these equations, we obtain

$$\tan^2 \tau = \tan^2 \lambda \left\{ \frac{\tan^2 \alpha \cos^2 \lambda + \tan^2 \beta \sin^2 \lambda}{\sec^2 \alpha \cos^2 \lambda + \sec^2 \beta \sin^2 \lambda} \right\},$$

or $\qquad \tan \tau = \tan \lambda . \sin \alpha \dfrac{\sqrt{1 - e^2 \sin^2 \lambda}}{\sqrt{1 - \sin^2 \varepsilon \sin^2 \lambda}}. \quad . \quad . \quad . \quad (76.)$

Let τ' be the segment of the second tangent between the point of *circular section* and the major axe, adopting nearly the same notation as in the latter case, we shall have

$$\cos \tau' = \frac{\sin \breve{x} \cos \eta \sec^2 \alpha}{\sqrt{\tan^4 \alpha + \tan^2 \breve{x}}},$$

and $\qquad \tan^2 \breve{x} = \dfrac{\tan^4 \alpha \cos^2 \lambda}{\tan^2 \alpha \cos^2 \lambda + \tan^2 \beta \sin^2 \lambda},$

$$\sin^2 \eta = \frac{\tan^4 \beta \sin^2 \lambda}{\tan^2 \alpha \sec^2 \alpha \cos^2 \lambda + \tan^2 \beta \sec^2 \beta \sin^2 \lambda}.$$

By the help of the last two equations, eliminating the functions of $\breve{x}$ and η, we find

$$\tan \tau' = \frac{\tan \lambda \tan^2 \beta \cos^2 \alpha}{\sin \alpha \sqrt{\dfrac{1 - e^2 \sin^2 \lambda}{1 - \sin^2 \varepsilon \sin^2 \lambda}}}; \quad . \quad . \quad . \quad (77.)$$

hence $\qquad \tan (\tau - \tau') = \dfrac{e^2 \sin \alpha \sin \lambda \cos \lambda}{\sqrt{1 - e^2 \sin^2 \lambda} \sqrt{1 - \sin^2 \varepsilon \sin^2 \lambda}}; \quad (78.)$

but this last expression is equal to $\tan v$, see (60.);
hence $\qquad\qquad\qquad \tau - \tau' = v,$
a result precisely similar to (44*.).

V. *On Parietin, a yellow colouring matter, and on the Inorganic Constituents of Lichens. By* Robert D. Thomson, *M.D., Lecturer on Practical Chemistry in the University of Glasgow**.

THE objects of the present paper are,—1st, to endeavour to show that, contrary to the generally received opinion, the class of plants termed Lichens require inorganic matter as part of their food, which they must derive from the localities upon which they are fixed; and, 2nd, to describe the yellow colouring matter obtained from the yellow wall lichen, and to detail its properties, composition, and application as a test for alkalies.

Although chemists are acquainted with several yellow colouring matters, few of them have been separated in a pure state and analysed. This may be satisfactorily explained by the circumstance that such substances are obtained with difficulty in the same state as that in which they existed in the plant from which they are extracted. The obstacle lies in the facility with which they unite with oxygen, and in their consequent conversion into a body of inferior beauty, and of an uncrystallized structure. The yellow colouring matters which have hitherto been analysed are derived from various parts of phenogamous plants, principally the roots and flowers. The subject of the present paper is obtained from a different class of plants—the lichens; but one to which we are indebted for some important colouring substances. The Greeks gave the name λειχὴν to a disease of the skin, and likewise to certain plants which were considered to possess the power of healing these cutaneous eruptions. Dioscorides tells us (Mat. Med. b. iv. c. 48) that the lichen, which is familiarly known from its growing on stones and attaching itself to the rough parts of rocks, like a moss, was called by some persons *bryon*, and was useful in the cure of sanguineous fluxes and inflammations. Pliny likewise employs the term lichen, but from his describing it as growing on rocks with one leaf from a broad root, and with one small stem, it is obvious he refers to a species of Hepatica (Nat. Hist. xxvii. c. 4). Galen likewise enumerates lichens among the instruments of cure in the treatment of impetigenous or cutaneous diseases. Modern botanists, up to a comparatively recent period, appear to have overlooked this class of plants, if we may draw this conclusion from the catalogue of English plants by John Ray, the second edition of which was published in 1677. In this work the celebrated author describes, under the title of lichen, eight

species of plants, only three of which, however, can be reck-
oned true lichens, the remainder being Hepaticæ and Algæ,
closely allied tribes. In Hooker's British Flora, published in
1833, there are described 420 species, included under 39
genera of lichens, as inhabiting the three kingdoms. It is well
known that many of these are capable of affording powerful
colouring matters.

The lichen from which the colouring matter to be described,
which I propose to term *Parietin*, is derived, is of very fre-
quent occurrence on walls and trees. It is the *Parmelia pa-
rietina* (yellow wall Parmelia), described by Hooker as having
the frond "orbicular, bright yellow; the lobes marginal, ra-
diating, appressed, rounded, crenate and crisped, granulated
in the centre; beneath paler and fibrillose, the repositories
being deep orange, concave, with an entire border." The
bright yellow colour of the lichen is a sufficient indication of
the presence of a colouring matter, but the real intensity of
the colour could scarcely be anticipated merely by an inspec-
tion of the plant.

Inorganic constituents of Lichens.

It is stated by Sir William Hooker, that among the lichens
" imperfect roots are sometimes formed, but more for the pur-
pose of fixing the plant to its place of growth than of deriving
nutriment, which appears to be afforded solely by the air"
(English Flora, vol. v. part 1. p. 129). And if we examine
the writings of other botanists, from the circumstance of the
occurrence of inorganic matter in lichens being particularized
in very few instances, and as being rather singular, the im-
pression produced is, that inorganic matter is by no means a
necessary ingredient in the constitution of lichens; oxalate of
lime, it is true, had been observed in several species of lichens,
but these were considered peculiar instances. Small portions
of bitartrate of potash and phosphate of lime had also been
detected in one or two species, but these examples do not ap-
pear to have led to any generalization, or even to the suspi-
cion that inorganic matter was of very frequent occurrence in
the tribe of lichens. I was not, therefore, prepared to expect
the remarkable results which the analysis of the yellow Par-
melia afforded. In one experiment 50 grains of the plant ob-
tained from the mica slate rocks of Dunson, on the west coast
of Scotland, when carefully washed, dried, and then ignited,
yielded 3·4 grains of inorganic matter; and in another expe-
riment 40 grains, carefully washed, as in the preceding trial,
afforded by burning a residue of 2·7 grains; in a third expe-
riment 7 grains of the carefully selected upper parts of fronds,

which had never been in contact with rock, after washing as in the previous instances, yielded by incineration 0·47 grain of a skeleton answering to the form of the lichen, and consisting of silica, iron, phosphates, &c.

I am thus particular in describing these experiments, made upon a small scale, because I found it extremely difficult when employing a larger amount to separate the extraneous particles which are always attached in considerable quantities to lichens inhabiting the surfaces of rocks. These three experiments give a per-centage of ashes amounting to

I.	II.	III.
6·8	6·75	6·71

Another specimen, consisting of the upper parts of fronds, afforded only 5 per cent. of ash, in which phosphate of alumina constituted a prominent ingredient. The ash in all these examples possessed the form of the lichen, as if the inorganic matter formed the skeleton upon which the organic portion was fixed. It sometimes effervesced slightly on the addition of an acid, and in one instance the carbonate of lime reached a considerable per-centage. On digesting the ash in water, some portion was dissolved; this solution, on the addition of chloride of barium, gave a white precipitate, part of which was insoluble in nitric acid. On throwing the sulphate of barytes on a filter and adding caustic ammonia to the filtered liquid, a flocky precipitate, phosphate of barytes, fell. The addition of an alcoholic solution of bichloride of platinum gave no indication of the presence of potash. Nitrate of silver gave a precipitate partially insoluble in nitric acid. The soluble salts, therefore, appear to be sulphate and phosphate of soda and common salt. The portion of the residue insoluble in water became nearly white when boiled with dilute muriatic acid, and left a gritty powder, which proved to be silica. The muriatic acid solution gave a copious precipitate with caustic ammonia; this precipitate was partly soluble in caustic soda, and consisted of peroxide of iron and phosphates of iron, lime and alumina. The results of the analysis of two specimens were as follows :—

	I.	II.
Silica	68·46	64·62
Soluble salts, consisting of sulphate, phosphate and muriate of soda	0·75	
Alumina and phosphate of alumina . .		0·83
Peroxide of iron and phosphates of iron and lime	22·04	34·55
Carbonate of lime	8·75	
	100·	100·

From these facts it is evident that this lichen requires the same inorganic constituents for food as other plants, with the addition that the amount of inorganic matter present in its composition is greater than in higher orders of plants, but in a proportion tending towards that existing in sea-weeds; another character, therefore, in addition to the general external features, indicating an alliance between the algæ and lichens.

It became now a subject of interest to ascertain if other lichens afforded analogous results. A number of experiments was made upon different species, the results of which are exhibited in the following Table:—

	Soluble salts. per cent.	Insoluble salts. per cent.	Total ash. per cent.
Cladonia rangiferina . .	9·75	2·71	12·47
Scyphophorus pyxidatus .			6·09
... ... bellidiflorus	0·59	0·59	1·18
Ramalina scopulorum . .	0·33	3·84	4·18
Parmelia omphalodes . .	0·33	7·79	8·12
... saxatilis . . .			6·91
... parietina . . .			6·75
Cetraria Islandica . . .			1·84

These inorganic substances being tested, yielded the same results as those described in reference to the constituents of the yellow Parmelia.

To ascertain if the inorganic matter was in any degree dependent on the rocks from which most of these specimens were procured, a specimen of *Parmelia saxatilis*, taken from the stem of an ash-tree ten feet from the ground, was incinerated and found to yield about 7 per cent. of ash, consisting of peroxide of iron and phosphates of iron, lime and alumina.

A specimen of *Parmelia saxatilis*, from rocks on the banks of Loch Venachar (where it is used, as well as generally in the Highlands of Scotland, with an alum mordant to impart a fine purple to woollen cloths), when ignited yielded only 3·9 per cent. of ash, a fact which shows, as well as many others which I have observed, that the amount of inorganic matter varies considerably in the lichens; and perhaps the same observation may without hesitation be considered applicable to all organic bodies.

It has been long remarked that lichens contribute essentially to the process of the disintegration of rocks, as they are "the first plants which clothe the bare rocks and form a humus for others of a higher organization to live and flourish in" (Hooker). The preceding experiments render it obvious that the lichens are enabled to produce this soil by extracting from the rocks upon which they are fixed, the ingredients re-

quired for the growth, both of themselves and of a higher order of plants. They may therefore be considered as producers of manure, or as constituting foci of matter necessary for the nourishment of plants.

Where lichens can be procured in abundance, a circumstance which may occur frequently in reference to the reindeer lichen, so frequent on our moors, they may be used with great advantage as manures, since they contain a larger amount of inorganic matter than any other plants, with the exception of the sea-weeds. The following Table contains my analyses of three kinds of wood, Lima, Sapan and logwood, from which it will be observed that the quantity of salts is much inferior to that contained in many of the lichens:—

	Lima wood.	Sapan wood.	Logwood.
Organic matter . .	971·255	987·083	971·400
Silica and sand . .	1·800		7·800
Common salt . . .		0·517	0·129
Phosphate and sulphate of soda . .	2·000	0·850	1·371
Phosphate of lime .	0·725		1·021
Carbonate of lime .	24·140	11·650	18·279
	1000·	1000·	1000·

The relative amount of inorganic matter in various classes of plants is apparent from the following Table, where the composition of 1000 parts of each of the plants is exhibited:—

	Lima.	Sapan.	Lichens.	Algæ.
Organic matter . .	971·25	987·08	932·5	762
Inorganic matter .	28·75	12·92	67·5	238

In this table the composition of the yellow Parmelia has been adopted, while in the algæ the result of the analysis of a gigantic sea-weed from Cape Horn, supplied me by Dr. Joseph Hooker, has been introduced. 490 grains of this plant yielded by incineration 116·7 of ashes, equivalent to 23·8 per cent.

The introduction of inorganic matter into the substance of trees and lichens can only be effected by the inferior extremity and surface of those portions which are in contact with the source of this peculiar pabulum of vegetable life, while it would appear that the connexion which we always find to subsist between sea-weeds and some fixed rocky position, even in the case of these immense inhabitants of the southern seas, according to some physiologists, only serves the purpose of retaining them stationary in one locality, their food being derived from the fluid in which they are immersed. But whether this be true or not, it is certain that the waters of the ocean are capable of affording all the inorganic constituents with which

these plants are supplied. Trees and lichens have no such atmosphere, rich in salt, from which they can derive their nourishment. They must be indebted for the inorganic food which they contain to the soil upon which they grow; thence, since lichens do certainly contain inorganic matter of various kinds, as appears from the facts detailed in this paper, the inevitable conclusion is forced upon us, that these species of plants are not only nourished by the atmosphere to which botanists have hitherto appeared to restrict their sources of food, but that they are also capable of extracting inorganic matter from the rocks and trees over whose surfaces this class of plants is so extensively distributed.

Preparation of Parietin.

When the yellow Parmelia is digested in cold alcohol, of spec. grav. ·840, a yellow fluid is produced, obviously from the solution of the yellow colouring matter of the lichen. When gently boiled the liquid becomes deeper coloured, and when a sufficient quantity of alcohol is employed and the liquor is allowed to evaporate spontaneously, the colouring matter is deposited on the sides of the vessel in the form of fine yellow needles, sometimes a quarter of an inch in length. The specimens of lichen from which crystals of this description were extracted, were derived from a dry wall, and at a distance from the neighbourhood of the sea.

In order to procure the colouring matter of the yellow Parmelia, it is proper to dry the lichen at a moderate temperature. This observation is particularly applicable to sea specimens, which are much more succulent than those derived from inland localities. By this precaution the alcohol will more completely extract the colouring matter, without violent or long-continued boiling. We should probably succeed in obtaining the purest product by removing as much as possible of the water from the lichen, by drying in a stove and then digesting in cold alcohol. The quantity of lichen, however, at my disposal has not hitherto been sufficient to enable me to accomplish the extraction in this manner. It has been stated that the colouring matter may be obtained in the form of needles, but generally it falls in the shape of brilliant yellow scales as the alcoholic solution cools. The mode in which it may be readily procured is, to boil the lichen gently with alcohol for a few minutes, and then to filter and add fresh alcohol until the colouring matter appears to be entirely taken up. The solution has scarcely passed through the filter before it begins to deposit the shiny scales of *Parietin*. If we attempt to purify these by redissolving them in alcohol, we shall find that

only a portion is dissolved, and the deposit from the alcoholic solution, instead of presenting the lustre of the substance as at first obtained, assumes the aspect of a brownish yellow powder.

Composition of Parietin.

The product of the second solution in alcohol, when dried at 212° and burned with oxide of copper, afforded the following results:—

$$3 \cdot 16 \text{ grains gave } 7 \cdot 376 \ CO_2$$
$$1 \cdot 410 \ HO.$$
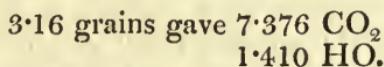
This corresponds with

		Experiment.	Calculated.					
Carbon ...	2·0116	63·65	40 atoms	63·82	40 atoms	62·51
Hydrogen	0·1566	4·95	16 ...	4·25	16 ...	4·16
Oxygen...	0·9918	31·40	15 ...	31·93	16 ...	33·33
	3·1600	100·		100·		

As it appeared from the preceding result that the parietin was altered in its character by attempting to redissolve it in alcohol, the parietin, after being extracted by alcohol from the lichen, was, after the filtration of this solution, allowed to deposit by cooling. It was then thrown on a filter and dried on a tile, and then digested in hot alcohol to remove any fatty or resinous matter with which it might be contaminated; or æther may be employed for the same purpose. The colouring matter thus prepared was dried at 212° and analysed.

2·96 grains afforded, when burned with black oxide of copper,

$$7 \cdot 15 \text{ grs. } CO_2$$
$$1 \cdot 294 \quad HO.$$
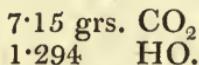
This is equivalent to

		Experiment.		Calculation.		
Carbon . .	1·9500	65·87	9 atoms	65·85
Hydrogen .	0·1437	4·85	4 ...	4·87
Oxygen . .	0·8663	29·28	3 ...	29·28
	2·9600	100·		100·

The formula, according to this view, would be $C_9 H_4 O_3$; or we may, as in the preceding case, consider it the oxide of an oil of the turpentine type, and its composition, when calculated, would be

Carbon	. .	40 atoms	65·21
Hydrogen	. .	16 ...	4·34
Oxygen	. .	14 ...	30·45
			100·
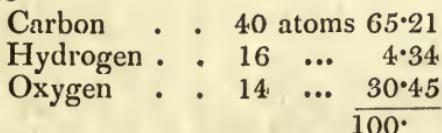
and the formula $C_{40} H_{16} O_{14}$, in which we have exhibited a stage in the oxidation of an oil similar to what we meet with in the gradual production of resins from the oils which appear

to constitute their bases. If we then consider parietin as ana-
logous to a resin derived from an oil, we may arrange the
preceding analyses as follows :—

<div style="text-align:center">

Oil of parietin (hypothetic) $C_{40} H_{16}$
Parietin . . . . . . $C_{40} H_{16} O_{14}$
Oxide of parietin . . . $C_{40} H_{16} O_{16}$

</div>

Parietin as a test for Alkalies.

The effect of reagents upon parietin is striking.   A very
minute portion of this substance will impart its yellow colour
to a large quantity of alcohol, and this solution is sensibly
acted on by reagents.   When to such a solution a drop or
two of nitric or muriatic or sulphuric acids are added, the
yellow colour imparted to it by the *parietin* becomes much
heightened, and even a very small proportion (much more
minute than that mentioned) will effect a sensible change.
When the solution is strong the addition of acid produces a
yellow precipitate.   When caustic ammonia, in the smallest
quantity, is dropped into, or applied by means of a rod, to a
solution of parietin, the yellow colour immediately becomes a
rich red, inclining to purple.   The same result is obtained
with caustic potash, caustic barytes, carbonate of soda, caustic
lime, &c.   The extreme delicacy of parietin in detecting alka-
lies suggests its utility in the laboratory.   An alcoholic solu-
tion may be kept for use, as the addition of a drop or two of
the solution to a considerable quantity of an alkaline liquor
will be immediately followed by a change to red; or the pro-
cess may be reversed, by placing a few drops of the alcoholic
solution in a test-glass, and adding to it a drop or two of the
alkaline liquor.   The alcoholic solution may be prepared
simply by digesting the lichen in cold alcohol of sp. gr. ·840,
as I have found that a small portion of lichen will impart a
colour to a large quantity of alcohol, sufficiently intense to
serve as a very delicate test for alkalies.   Observing the strong
colour which the alcoholic solution imparted to the filtering
paper which was used to purify the solution when first pre-
pared, I cut these into test-papers, and found that when pro-
perly impregnated with the solution, they were little, if at all,
inferior to turmeric paper in their delicate detection of am-
monia.   Test-paper may be prepared extemporaneously from
the alcoholic solution, when it is wished to detect ammonia,
by dipping a piece of paper into the alcoholic solution and
then applying it in its wet state to the ammoniacal vapour.
The yellow colour is immediately transformed into a reddish-
purple tint, more distinct than the colour that becomes appa-
rent in turmeric paper of old preparation under similar cir-

cnmstances, which is a dirty brown.   One of the principal re-
commendations of the liquid, not already noticed, is, the cir-
cumstance of its being capable of preservation without under-
going deterioration, while the test-papers, which have been
frequently recommended, although possessing most delicate
testing powers when freshly prepared, gradually lose their
value by preservation.   I believe this to be the explanation of
the failure, in this country, of some continental test-papers
which have been recently recommended.   It would therefore
appear that the best test-paper being that which is of fresh
preparation, the most convenient source for its production is
that from which the colouring substance can be most rapidly
procured in an efficient state.   The observations which have
been made upon parietin, in reference to its colouring powers,
tend to show that it may be employed with advantage for the
most delicate purposes to which turmeric is applied. Parietin,
however, is not acted on by acids ; the natural yellow colour
merely becomes brighter, while turmeric, which contains a
blue and yellow colouring principle, has the former reddened
by acids and the latter converted to a brown by alkalies.
Moistened yellow *parietin* paper, on the other hand, becomes
red or purple when freshly prepared—reddish-brown, if long
prepared—by coming in contact with ammonia and other al-
kalies.   The other reactions of parietin are simple.   The alco-
holic solution is precipitated yellow by nitrate of silver, acetate
of lead and other metallic salts.   A solution of permuriate of
iron renders the colour much darker.   The precipitates with
silver and lead have not been analysed, from the minute quan-
tity of parietin at my disposal.

The yellow colour of the *Parmelia parietina* early attracted
the attention of those persons interested in dyes.   It was ac-
curately described by Hoffmann, Amoreux and Willemet, in
1786*.   The latter informs us that the Swedes in the province
of Oeland obtained, by means of this lichen and alum, a yellow
dye for woollen stuffs, and that a flesh tint was also procured
from it fitted for linen and paper ; that goats eat this lichen,
and that Haller recommended it as a powerful tonic in diar-
rhœa.   He adds, that he had himself used it in his practice as
a tisan, and had found it to prove beneficial in that form of the
disease which occurs in autumn.   Hoffmann states, that in
Norway, when boiled with milk, it is used as a remedy in jaun-
dice. This idea may have, perhaps, originated from the corre-
spondence in colour of the disease and the remedy ; upon the

* Mémoires couronnés en l'année 1786 par l'Académie des Sciences,
Belles Lettres et Arts de Lyon, sur l'Utilité des Lichens dans la Médecine et
dans les Arts, 8vo, 1787.

principle so much in vogue at present, "similia similibus curantur." Hoffmann affirms that he never could obtain a yellow colour from this lichen, but that with wine-vinegar he obtained an olive-green or fawn colour, and with true wine-vinegar (aceto vini vero) and copperas a flesh or apricot shade. Of these colours he has appended to his essay specimens, together with forty-nine others obtained from various species of lichens. Dr. John P. Westring, of Nordkoping in Sweden, who prosecuted an extensive inquiry into the colouring matters of lichens, describes the *Lichen parietinus* (Wagglaf) as affording with wool, by infusion for fourteen days and then boiling for half an hour, a fawn colour; by longer boiling a yellow was produced, and this mixture became, by simple infusion and extraction, similar to the red wool of Florence. With common salt and nitre, boiled for an hour, a beautiful straw colour was elicited. Upon silk it gave similar colours, differing in their shade from red to yellow, according to the methods employed in dyeing the goods*.

Subsequently to these observations, which are perhaps interesting in an œconomical point of view, the yellow Parmelia was recommended by Dr. Sande, probably misled by the colour, as a substitute for Peruvian bark, during the last French war. It has also been chemically examined by Herberger, but not apparently with the same results afforded by Scotch specimens, as he found no inorganic constituents, which amount to from 6 to 7 per cent. according to my trials, and obtained a much larger quantity of colouring matter than existed in any plants examined by me. He also found a red colouring matter which did not appear in the process of extraction as followed by me, and which may, therefore, be a product of the oxidation of parietin.

More lately still Dr. Gunprecht extracted yellow oil from the lichen, but in such minute quantity as not to be susceptible of examination. In my own experiments I always found that the colouring matter volatilized when the lichen was heated, just before it caught fire, giving rise to abundant yellow fumes. I have also procured a considerable quantity of sugar in crystalline grains, when the alcohol, with which the colouring matter was extracted, was allowed to evaporate spontaneously.

Glasgow, November 1843.

Note.—Since the preceding paper was read, and before I could procure a sufficient quantity of the yellow needles above mentioned for analysis, they have been examined at Giessen by Rochleder and Heldt, and found to consist of $C_{40} H_{16} O_{12}$, so that we have now the following oxides:—

* *Kongl. Vetenskaps Acad.* xii. p. 300. ann. 1791.

Oil of parietin	.		$C_{40} H_{16}$
Parietic acid .	.		$C_{40} H_{16} O_{12}$
Parietin	.	.	$C_{40} H_{16} O_{14}$
Oxide of parietin			$C_{40} H_{16} O_{16}$

I have found parietin in the *Squamaria elegans* (brought from Cockburn Island, in 64° S. lat., by Dr. Joseph Hooker, and styled by him "the most antarctic plant"), a fact which adds strength to the idea of Mr. Griffith, that many of the coloured lichens, such as *Lecanora vitellina* and *concolor, Squamaria murorum, elegans,* &c., are probably the *Parmelia parietina* under different circumstances, as all these plants seem to owe their colour to the presence of parietin.

VI. *On Mr. Hunt's Book, entitled " Researches on Light."*
By Professor DRAPER.

To the Editors of the Philosophical Magazine and Journal.
GENTLEMEN,

A COPY of Mr. Hunt's book, entitled " Researches on Light," has been sent to me from London, and I sincerely regret that I am called upon to express myself in disapproval both of its contents, and of the spirit in which it is written.

I must enter my protest against this book being received as any authority in the science on which it professes to treat; either as a work of history, or as an exposition of known facts.

In a treatise which commences with the history of light from the writings of Moses, and which professes to be "the first history of photography which has been published," giving " to every one his full share in those discoveries which have accelerated the advancement of the art," *there is not one word in allusion to the application of the Daguerreotype to the taking of portraits from the life ; unquestionably the most important application of the art which has yet been made, and which Mr. Hunt knows belongs to me.*

There is scarcely an experiment of mine, to which allusion is made, which is not either misrepresented or misunderstood. Of the experiments of others I have nothing here to say.

Against the spirit in which this volume is written I must also protest. Any person who reads it through, without a previous acquaintance with these matters, would rise from the perusal of it with an impression that the science of photography is the intellectual creation of Mr. Hunt,—that some very good contributions to it have been made by Sir John Herschel, and a few remarks here and there added by distant or obscure experimenters.

Now, Sir, this is all wrong. It is time that scientific men

should set their faces against these things. Twice in the brief history of photography have these unpleasant occurrences happened. Egerton published a treatise* in which the names of Sir J. Herschel and Mr. Hunt were scarcely to be seen.

The air of vanity and the curious errors which pervade this book will impose on some and amuse others. But there is one point to which I wish *particularly* to direct the attention of your readers. As is well known to them, I have ever since 1842, written from time to time papers to prove the existence of a fourth imponderable ; I gave it a name, and have incorporated that name prominently in all my writings. Before me no person had ever hinted that the well-known chemical rays constituted a new imponderable, which was entitled to the same rank as light, heat, and electricity. It is now too late for any attempt to be made to deprive me of the right of proprietorship over that capital fact. *Yet Mr. Hunt, in this book, actually appropriates it to himself, and gives the fourth imponderable the name of* ENERGIA.

Had Mr. Hunt represented his treatise as giving his *own* researches and opinions, it would have answered to its name ; but, when he sets it forth as containing all that is known up to the present time, he does a great injustice to almost every one who has written on these matters. For myself, of the different facts I have published in this and other Journals, nine-tenths are not so much as alluded to. A person who should content himself with the information here given, would scarcely have an idea of the present advances of the science, and would mistake it for a heterogeneous collection of isolated facts.

I hope such of your readers as take an interest in photography, will look at that chapter which commences on the 259th page, and also at the concluding paragraph of the preceding one. Let me ask them, whether they would not infer that the idea of the existence of a fourth imponderable was now in 1844 brought forward for the first time by Mr. H. Then let them turn to the papers written by me in 1842, and notice the same arguments used for the same purpose, and this very doctrine emphatically set forth (Phil. Mag. Dec. 1842). If the original observer of facts,—the original propounder of an important idea, is to have his rights thus unceremoniously put aside (and should the world of science be led to countenance the proceeding), there is an end of every inducement to philosophical pursuits.

The chemical rays were discovered in the last century. The idea that they constitute a fourth imponderable of the same rank as light, heat, and electricity, belongs to me. That

* A translation of Lerebours's Treatise, with additions.

idea may prove to be false; but should it prove to be true, I have the same undoubted right to impose on that imponderable a name, which Davy exercised in giving currency to the word chlorine and its derivatives. The cases are parallel. Has not the universal receipt of that name settled the law?

I have now quoted two prominent instances with a view of stamping a proper character on this work. I might have increased these observations to a multitude of other cases which are just as bad; but my purpose is answered.

University, New York,   Yours, &c.,
May 14th, 1844.   JOHN W. DRAPER.

VII. *Observations on the Diffusion of Gases.*
By T. S. THOMSON, *Esq.*
To Richard Phillips, F.R.S., &c.

DEAR SIR,

THE diffusion of gases is a subject which has agitated the minds of chemical philosophers ever since the elementary facts were noticed by Dr. Priestley. Dr. Dalton, in his New System of Chemical Philosophy, thus expresses himself, part 1. p. 150:—" When two or more elastic fluids, whose particles do not unite chemically upon mixture, are brought together, one measure of each, they occupy the space of two measures, but become uniformly diffused through each other, and remain so whatever may be their specific gravities. The fact admits of no doubt; but explanations have been given in various ways, and none of them completely satisfactory.

" Dr. Priestley was one of the earliest to notice the fact; it naturally struck him with surprise, that two elastic fluids, having apparently no affinity for each other, should not arrange themselves according to their specific gravities, as liquids do in like circumstances. Though he found this was not the case after the elastic fluids had been once thoroughly mixed, yet he suggests it as probable, that if two of such fluids could be exposed to each other without agitation, the one specifically heavier would retain its lower situation. He does not so much as hint at such gases being retained in a mixed state by affinity. With regard to his suggestion of two gases being carefully exposed to each other without agitation, I made a series of experiments expressly to determine the question, the results of which are given in the Manchester Memoirs, vol. i. (New Series). From these it seems to be decided that gases always intermingle and gradually diffuse themselves amongst each other, if exposed ever so carefully; but it requires a considerable time to produce a complete intermixture, when the surface of communication is small. This time may vary from

a minute to a day or more, according to the quantity of the gases and the freedom of communication."

Dr. Dalton proceeds further to discuss the rationale of the action, and the result was (to use his own words), " In the autumn of 1801 I hit upon an idea which seemed to be exactly calculated to explain the phænomena of vapour; it gave rise to a great variety of experiments, upon which a series of essays were founded, which were read before the Literary and Philosophical Society of Manchester, and published in the fifth volume of their Memoirs, 1802.

" The distinguishing feature of the new theory was, that the particles of one gas are not elastic or repulsive in regard to the particles of another gas, but only to the particles of their own kind."

The immediate result of the publication of this view of the mutual relations of mixed elastic fluids was, a discussion between Dr. Dalton, Mr. Murray, Mr. Gough, &c., which terminated in leaving the subject of " diffusion " in a state of indecision.

Mr. Graham took up the subject experimentally in 1825, and ascertained the arithmetical law of the rate of diffusion, which he thus expresses:—" The mathematical relation which subsists between the diffusion, volume and the density of a gas is expressed thus:

$$\text{diffusion volume} = \frac{1}{d^{\frac{1}{2}}},$$

where *d* represents the specific gravity of the gas."—*Elements of Chemistry,* p. 72.*

Mr. Graham's experiments were performed with great care, and are of undoubted accuracy.   In 1834, through the medium of your valuable Magazine, I offered to the public some remarks upon Mr. Graham's Law of the Diffusion of Gases, endeavouring to reconcile Mr. Graham's facts with Mr. Dalton's theory.   Mr. Graham, in his Elements of Chemistry, p. 75, thus notices my remarks:—" The law of the diffusion of gases has on this account been viewed by my friend, Mr. T. S. Thomson of Clitheroe, as a confirmation of Dr. Dalton's theory, that gases are inelastic towards each other†.   It must be admitted that the ultimate result in diffusion is in strict accordance with Dalton's law, but there are certain circumstances which make me hesitate in adopting it as a true representation of the phænomenon, although it affords a con-

* [On the law of the diffusion of gases we refer those who may pursue the subject to Faraday's Experimental Researches in Electricity, Series vi. par. 659.—EDIT.]
† Phil. Mag. S. 3. vol. iv. p. 321.

venient mode of expressing it. 1. It is supposed, on that law, that when a cubic foot of hydrogen gas is allowed to communicate with a cubic foot of air, the hydrogen *expands* into the space occupied by the air, as it would do into a vacuum, and becomes two cubic feet of hydrogen of half density. The air, on the other hand, expands in the same manner into the space occupied by the hydrogen, so as to become two cubic feet of air of half density. Now of the gases actually expanded through each other in this manner, cold should be produced, and the temperature of the mixed gases should fall 40 or 45 degrees, but not the slightest change of temperature occurs in diffusion, however rapidly the process is conducted. 2. Although the ultimate result of diffusion is always in conformity with Dalton's law, yet the diffusive process takes place in different gases with very different degrees of rapidity. Thus the external air penetrates into a diffusion tube with velocities denoted by the following numbers, 1277, 623, 302, according as the diffusion tube is filled with hydrogen, with carbonic acid, or with chlorine gas. Now, if the air were rushing into a vacuum in all these cases, why should it not always enter it with the same velocity? Something more, therefore, must be assumed than that gases are vacua to each other, in order to explain the whole phænomena observed in diffusion."

I cannot admit the validity of either of the objections. In the first place, as to the production of cold, Dr. Dalton has ascertained and published the facts of the case. If air of pressure 30 inches of mercury is allowed to rush into a vacuum, cold is produced in the vessel from which the air rushes, and a momentary rise of temperature is observed in the vacuum into which it rushes. This disturbance of the equilibrium of temperature only lasts a few seconds, and it was solely by the rate at which the thermometer descended and ascended that Dr. Dalton inferred the "fall of 40 or 45 degrees."

If oxygen is exposed to hydrogen, on which side is the reduction of temperature to take place? I think on neither ; if they began to heat each other, they would naturally at the same time begin to cool each other. If you let a cubic foot of hydrogen loose into a vacuum it goes in at the rate of sound, 1142 feet in a second ; but if the vacuum is pre-occupied with oxygen it requires longer time, and any variation of temperature would be unobservable. As to Mr. Graham's objection, No. 2, I have only to observe, that I never committed myself so far as to say that when air was rushing into hydrogen or carbonic acid, or chlorine, it was "rushing into a vacuum."

In my explanation of the phænomena of diffusion on Dr. Dalton's theory, investigating the results of the relative velocities of different gases into a vacuum and deducing the pre-

viously unnoticed fact, that for all gases the moving force of the issuing current is the same, I expressly state, in proceeding to apply the formula

$$\varepsilon\, m = \varepsilon'\, m'*$$

to diffusion, that "instead of issuing into a vacuum, they should be permitted to diffuse into each other," &c.   To meet Mr. Graham's objection, however, I refer to his experiments (p. 74, Elementary Chemistry), where he says, " A slight deviation from the law is observed in gases which differ in a certain physical property from air, namely in the greater facility with which they may be forced through pores or minute apertures by pressure."

. Mr. Graham finds that carbonic acid and hydrogen rush through stucco into an exhausted receiver, attaining the same internal tension in ten minutes and four minutes respectively. This I conceive to be the reason why, when in a diffusion tube, the air enters against hydrogen more rapidly than against carbonic acid ; the hydrogen comes out more quickly than the carbonic acid, and the conditions of the law being fulfilled, the air rushes in more rapidly, ten minutes to four being very nearly in the proportion of 1277 to 623.

My immediate object in addressing you, however, is to say that I have a new idea on the subject, which with diffidence I submit to the public.   I am only induced to do so because it has in some degree the sanction of the late James Ivory, Esq., to whose letters to me on the subject I refer you.

I give up the idea of the mutual inelasticity of the particles of different gases.   As Mr. Ivory says, "though it is intelligible in words, it is incomprehensible to the understanding."   I am inclined to attribute diffusion to an attractive force of a peculiar character ; in one sense a *chemical* force, because it exists only between the particles of *dissimilar* gases ; in one sense a *mechanical* force, because it obeys a mechanical law,—the law of diffusion as ascertained by Mr. Graham.   The atmosphere of heat surrounding a particle of oxygen is as repulsive to the atmosphere of heat surrounding a particle of hydrogen as it is to that surrounding another particle of oxygen ; but beyond this atmosphere is an incipient chemical attraction of oxygen for hydrogen, not intense enough to determine a direct combination and the formation of water, but still sufficient to determine the particles of oxygen to diffuse themselves gradually among the hydrogen, and *vice versâ*.

A simple diagram will illustrate this better than any verbal description.   Consider two particles of oxygen (✳) presented to two particles of hydrogen (◎).

* ε, ε' representing the *volumes* of the interchanging portions of diffusing gases, and m, m' their *masses*.
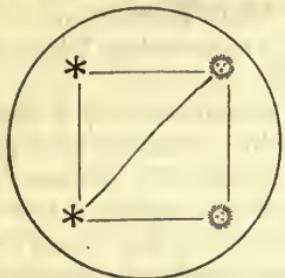
Before diffusion.   After diffusion.
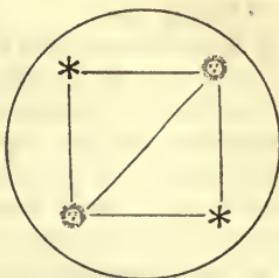
After diffusion the dissimilar particles have obviously on the whole approached each other, and the similar ones receded from each other; what is true of two atoms is true of any number.

It is curious that the same mathematical formula by which I expressed the conditions of the question on the inelastic hypothesis are equally applicable to the attractive theory.

It is an instance of Newton's third law of motion, that action and reaction are equal, and the very same formula applies to attractive as to repulsive forces, simply by the change of sign (*plus* or *minus*) + or −.

There are many collateral applications of this idea, for the explanation of which I shall at some future period again seek the medium of your valuable Journal. In conclusion, I present you for publication two letters connected with this subject, addressed to me by the late James Ivory, Esq., and subscribe myself,   Dear Sir,

Your sincerely obliged friend,

London, June 11, 1844.   Thos. S. Thomson.

" 121 Camden Road Villas, Camden Town,
" My dear Sir,   February 21, 1842.

"I am much gratified by your letter. I have only added to Dalton's theory what is necessary to reduce it to correct physical principles. In the next Magazine in the press, you will find that I have applied the theory as I have explained it, to determine the composition of atmospheric air; that is, I have demonstrated as a consequence of the theory that a volume of air consists of 21 parts of oxygen and 74 of azote. In the Magazine for April I propose to continue the subject, chiefly to explain the manner in which aqueous vapour exists in the atmosphere, which comes under the same theory.

"I shall be very glad to have the perusal of your speculations.   " Believe me to be,

" *To Thos. S. Thomson, Esq.*, " Respectfully, yours, &c.,
  5 *Bury St., St. James's.*"   "James Ivory."

<div align="right">
" 121 Camden Road Villas, Camden Town,
March 7, 1842.
</div>

" DEAR SIR,

" I have to apologize for not writing sooner, but I have had a smart attack of asthma, which is now gone off.

" Graham's experiments are very ingenious, but I do not see that they can be applied to explain the diffusion of gases as laid down by Dalton, and now universally admitted. I consider gases and steam (so long as it is steam) follow exactly the same laws, the only difference being that steam liquefies at an accessible temperature and pressure, whereas the gases liquefy at temperatures and pressures that are almost inaccessible. I view all gaseous bodies as elastic fluids that act upon one another and upon other bodies by the laws of elastic fluids ; and this seems to be sufficient to account for all the physical properties discovered by experiment. It also gets rid of the only exceptionable part of Dalton's theory, namely his hypothesis that one gas is inelastic to all other gases, which, although it be intelligible in words, is incomprehensible by the understanding.

" I do not apprehend that your manner of explaining Graham's experiments is liable to objection.

" *To Thos. S. Thomson, Esq.*, " Believe me to be, yours, &c.,
5 Bury St., St. James's."   " JAMES IVORY."

VIII. *On the Cyanides of the Metals, and their Combinations with Cyanide of Potassium*. Part I. *Cyanide of Gold.* *By* Messrs. CHARLES F. O. GLASSFORD *and* JAMES NAPIER*.

1. THE various compounds which cyanogen forms with the metals constitute one of the most interesting class of bodies which the science of chemistry can produce; and the rapid progress of electro-metallurgy, with the almost universal use of these cyanides in the practice of this art, adds a double interest to the study of these compounds with the alkaline cyanides. Indeed, we might have expected that long before this some definite information would have been published upon the nature, constitution and easy preparation of these salts, as yet however, so far as we are aware, nothing complete has appeared. With a view to remedy this, and to facilitate a full investigation of the subject, we have drawn up the following observations, the result of extensive practical operations in the art of electro-metallurgy, which we beg to present to the Chemical Society, in the hope that our results, in connexion with those of others who may be labouring in the

* Communicated by the Chemical Society; having been read February 19, 1844.

same field, may lead to a more precise knowledge of their character and constitution.

2. In systematic works on chemistry reference is made to this subject in a very general way. The sum of such information may be thus expressed:—" The soluble cyanides of the alkaline metals, potassium, sodium, &c., dissolve the insoluble cyanides of the metals proper, forming double salts, which are generally crystallizable :" the probable constitution of only a very few of these salts are given.

3. From the researches of L. Gmelin upon the metallo-cyanurets, which were published many years ago, it would appear that there are two classes of compounds in which other metals are found to replace the iron in the yellow and red prussiate of potash, and to form indeed metallo-cyanurets and sesquicyanurets of potassium, &c. There seems however to be a singular indefiniteness on this subject in chemical works ; for Turner, in his ' Elements,' 5th edition, p. 779, says, that " L. Gmelin has obtained a double cyanuret of zinc and potassium, in which the zinc acts as iron in the ferro-cyanurets." He also says, " that a cobalto-cyanuret of potassium, and similar double cyanurets of nickel and cobalt, may be prepared in the same manner as the preceding salt;" and further, " that Gmelin procured the cobalto-cyanuret in yellow crystals, similar in composition and form to ferro-cyanuret of potassium." The salts here described are evidently analogous to the yellow prussiate of potassa, or ferro-cyanuret of potassium ; but on referring to Professor Graham's ' System,' we find there described salts of cobalt, chromium and manganese, not analogous to yellow prussiate of potash, but similar in symbolic constitution to the red prussiate of potash, that is, metallo-sesquicyanurets having a supposed salt radical in which these metals replace iron to the formation of cobalti-, chromi-, and mangani-cyanogen ; no mention is made whatever of the existence of compounds similar to yellow prussiate of potash, or of a cobalto-, chromo-, and mangano-cyanogen.

On turning to Berzelius's *Traité*, tome ii. p. 115, we find a description of the same zinco-cyanuret of potassium, or, as he names it, cyanuri zincoso-potassique, analogous in composition to the yellow prussiate of potassa, in which zinc is found to replace iron. He says, " The existence of this salt, of which the discovery is due to L. Gmelin, leads us to presume that zinc has a series of double cyanurets, like iron, nickel, &c." Here also Berzelius corroborates the statement made by Dr. Turner, and speaks of these compounds as analogous to the yellow prussiate of potassa, or as metallo-cyanurets. Turner mentions (p. 780) that Gmelin's observations upon the nickel, copper and zinco-cyanurets have been confirmed by a late

pupil of his, Mr. F. Rodgers, and his brother; on referring to their Memoir, published in the Philosophical Magazine of February 1834, we find a different statement from what Dr. Turner makes; they say, " L. Gmelin prepared a double cyanuret of cobalt and potassium, analogous to the red cyanuret of iron and potassium." These different statements of apparently the same thing must have arisen either from the want of proper care in naming the salts, so as to indicate to which class they belong, or to the want of sufficient evidence respecting their constitution. We may render this still more evident by the following statements. Berzelius mentions a cyanure platinoso-potassique, or platino-cyanuret of potassium, but on referring to its alleged constitution, we do not find, as its name indicates, a compound of platina similar to the ferro-cyanuret of potassium, but a compound totally different from either the yellow or red prussiate of potassa ; the same salt is described in Graham's ' Elements,' p. 991 ; and there also it is called the platino-cyanide of potassium. The acid described in some chemical works as the platino-cyanic acid, does not resemble in constitution the ferro-cyanic acid, as its name indicates. If, therefore, there be a necessity for distinguishing the acid formed by the supposed salt radical ferri-cyanogen with 3 eqs. of hydrogen, from that formed by ferro-cyanogen with 2 eqs. of hydrogen, so then there is an equal necessity for distinguishing a metallo-cyanogen, which combines with 2 eqs. of hydrogen, from a metallo-cyanogen, which combines with 1 eq. of hydrogen : for the same reason also platino- and other cyanides of potassium should receive appellations which indicate their constitution, and not such as by analogy lead us to infer a totally different constitution. These remarks tend to show us the necessity of carefully re-examining the nomenclature of this apparently complex subject, and the scrupulous care with which we ought to classify and name these compounds, so as neither to misrepresent their constitution, nor to leave the possibility of their being ascribed to classes to which they do not belong.

4. We deem these observations necessary for the purpose of alluding to a memoir by Mons. A. Meillet, which appeared in the *Journal de Pharmacie et de Chemie* for June last, and which was translated into the Philosophical Magazine for August, entitled, " On some new combinations of Cyanogen." The author says, " The peculiar manner in which cyanogen acts towards iron, by forming two very stable acids with it, leads to the supposition that it is not the only metal with which cyanogen is capable of combining. In fact, some German chemists, and Gmelin among others, have discovered three new compounds, which are platino-cyanogen, cobalto-

cyanogen, and chromo-cyanogen, and afterwards the hydro-
genated acids analogous to ferro-hydrocyanic acid, and seve-
ral other metallic salts." Here again we have more evidence
of the singular confusion with which this subject has become
invested. If we are to accept these names for what they ex-
press, and as yet we have no reason to do otherwise, we must
infer that these metallo-cyanogens are analogous to the ferro-
cyanogen; but this is not the case, for we have just shown
that platino-cyanide of potassium is not similar in constitution
to the ferro-cyanide of potassium. Meillet then proceeds to
say, " that the processes employed by these chemists were
somewhat complicated, and they had not continued their ex-
periments;" the method which he employed, he says, " is
simple, and there may be procured by it a great number of
perfectly definite compounds." The remainder of the paper
is taken up with a brief description of the characteristics of
these salts; among others he mentions the auro-, the argento-
and the cupro-cyanides of potassium, classifying these under
their supposed salt radicles, as auro-cyanogen, argento-cya-
nogen, and cupro-cyanogen. It is in the consideration of these
various metallic combinations with cyanogen, that we have
made the following investigations. Before commencing the
detail of these, we must offer a few observations upon the
cyanide of potassium as usually prepared, and as employed by
Meillet in his experiments.

5. The cyanide of potassium which is employed by us in
our daily operations is prepared according to the process first
pointed out by Messrs. F. and E. Rodgers, now generally
known by the appellation " Liebig's Process;" from what
particular cause it came to be so named we know not, unless
it may have been that the publication of this process by Pro-
fessor Liebig occurred at a time when the attention of prac-
tical and scientific men were more particularly drawn towards
an attentive consideration of this compound, from its important
applications in chemical analyses, and its extensive employ-
ment in the new art of electro-metallurgy. To show, how-
ever, that this process was known long before this illustrious
chemist pointed it out, we may be allowed to quote the fol-
lowing passage from the memoir of Messrs. Rodgers, already
mentioned as having been published in the Philosophical Ma-
gazine for February 1834. After detailing the various pro-
cesses then known, and others which they had discovered, they
describe this process as follows:—" Cyanuret of potassium
may be prepared by exposing a mixture of anhydrous car-
bonate of potash and anhydrous ferro-cyanuret of potassium
to a moderate red heat, in a covered porcelain crucible, for
about twenty minutes." " The proportions employed should

be nearly 1 eq. of anhydrous carbonate of potash to 1 eq. of the anhydrous ferro-cyanuret of potassium. The process is very productive, for the weight of the cyanuret of potassium obtained by this process is much greater than the weight of the cyanuret obtained by heating ferro-cyanuret of potassium alone, owing to the cyanogen of the cyanuret of iron uniting with the potassium contained in the carbonate of potash ; and, moreover, the process occupies less time. Some carburet of iron always remains in the crucible."

6. Cyanide of potassium may be obtained pure for experimental investigation in the laboratory by the use of absolute alcohol ; but where hundred weights are prepared and used at a time this is not admissible. As employed in the arts it contains many impurities: these are chiefly cyanate, carbonate, muriate and sulphate of potash, silica, and yellow prussiate of potash. The average amount of these impurities is 35 per cent., often as high as 50 per cent. It contains much silica when prepared, as is usually directed, in earthenware crucibles ; and when the exact proportions of the yellow prussiate of potash or carbonate of potash are not employed ; when these are not perfectly anhydrous, salts of ammonia are generated ; and when commercial carbonate of potash is used, muriate and sulphate of potash. Even when the materials are employed chemically pure and perfectly anhydrous, it contains 20 per cent. of cyanate of potash ; this, according to Professor Liebig, is of little consequence in the ordinary operations of testing, but in respect to analytical inquiry, and to electro-metallurgical operations, we find it of the greatest importance. As however we shall have opportunities of pointing these out in detail, we only allude to them here in a general way.

7. When nitrate of silver is added to solution of cyanide of potassium, just so long as the precipitate which is at first formed is redissolved, we obtain the *whole* of the cyanide of potassium in union with the silver, none of the other salts present, although amounting to a large per-centage, taking any part in the action. Upon this law we have founded a method of estimating the exact quantity of pure cyanide of potassium in any sample ; as, however, the principle upon which this depends can be better explained when the nature and constitution of the cyanide salt of silver is described, we shall return to it when treating of that metal.

8. As when one eq. of chloride of gold combines with one eq. of chloride of potassium, a double salt, called chloride of gold and potassium, is formed, so shall we call the union of one eq. of cyanide of gold with one eq. of cyanide of potassium the "cyanide of gold and potassium," and apply similar names to the other metallic salts having a like constitution. The

terms double-cyanides, bicyanides, &c., which are usually given to such compounds, we consider neither necessary nor sufficiently expressive.

9. The equivalent numbers employed are those given in Dr. Turner's ' System,' namely,—Cyanogen 26·39, potassium 39·15, cyanide of potassium 65·54, gold 199·20, cyanide of gold 225·59.

10. *Cyanide of Gold.*—Little is said in any systematic treatise of the cyanide of gold. Berzelius, almost the only chemist who mentions this compound, says, " That this salt presents itself under the form of a pale yellow substance, insoluble in water, which is obtained by precipitating the chloride of gold with cyanide of potassium, and gives as its composition the formula Au, Cy$_3$." Kane also mentions this pale yellow compound, and gives it the same composition. It may readily be obtained by decomposing the chloride of gold with cyanide of potassium, but both solutions must be saturated, and care must be taken not to add an excess of cyanide of potassium ; the product however by this process is very small compared to the quantity of gold employed, and it is necessary to proceed as follows to obtain the whole of the gold as cyanide :— Saturated solutions are to be used, and a slight excess of cyanide of potassium added, so as to redissolve the cyanide of gold; the whole is to be evaporated to perfect dryness, treated with nitric or muriatic acids to decompose the cyanide of potassium, again evaporated to dryness, and gently heated so as to expel the excess of acid, when cold water is to be added, and the whole thrown upon a filter and thoroughly washed. Obtained in this way however it generally contains a portion of silica, particularly if the cyanide of potassium has been prepared in earthenware crucibles; and to obtain the cyanide pure we have recourse to other methods. The best process is to decompose the cyanide of gold and potassium. The salt is to be dissolved in a small portion of distilled water, nitric or muriatic acid added, and the liquid brought to a boiling heat, the cyanide of gold then precipitates; but it is necessary to expel the excess of acid by evaporation before the whole of the cyanide is obtained; the dry mass is then treated with water, thrown upon a filter and well washed.

The cyanide of gold obtained by the last method is of a beautiful canary yellow colour, which is not affected by exposure to light, or by the action of acids: it is an exceedingly fine powder, and readily passes through a single filter; it is insoluble in caustic potash and water ; is slightly soluble in nitric, muriatic, and sulphuric acids when recently precipitated ; is soluble in ammonia, hyposulphite of soda, and readily so in cyanide of potassium; when fused at a red

heat with carbonate of potash, it is entirely decomposed, and
the gold may be obtained in a bright metallic button.

11. The cyanide obtained by the decomposition of the double
salt by acids was examined in this way and the following re-
sults obtained:—

    10 grs. yielded 8·82 grs. of gold = 88·20 per cent.

    15 grs. ...   13·23 grs.   ... = 88·20 ...

    7 grs. ...   6·17 grs.   ... = 88·14 ...

These results approximate so very closely to the calculated
result, viz. 88·30 per cent., that we are at once led to the con-
clusion that it is a true proto-cyanide, the constitution of
which is represented by Au + Cy.

A cyanide of the constitution represented by Berzelius and
Kane would contain only 71·56 per cent.

The cyanide of gold obtained by the first process is gene-
rally contaminated, as already mentioned, with silica and a
small portion of iron, obtained from the cyanide of potassium
employed: these impurities will vary according to the state
of the cyanide used. Before these facts were ascertained the
analyses of the cyanide of gold yielded the following very
variable results,—85·00 per cent., 85·50 per cent., 85·57
per cent.; this last sample, when thoroughly washed with
water, carefully dried and examined, gave still only 86·00 per
cent.

12. *Cyanide of Gold and Potassium.*—This salt, which is
that now so extensively employed in gilding by the electric
force, may be obtained by various processes:—1. By dissolving
the cyanide of gold, prepared by the first process just given,
in cyanide of potassium. 2. By dissolving oxide of gold in
cyanide of potassium. The oxide of gold is prepared for this
purpose by digesting calcined magnesia in nitro-muriate of
gold, as is well known; as it is very apt to retain a portion of
magnesia as usually prepared, it is better to boil the oxide so
obtained in strong nitric acid, to filter through close muslin or
calico, allowing the green nitrate solution to fall into a large
quantity of distilled water. The oxide of gold is thus ob-
tained quite pure, and as a very bulky reddish-brown pre-
cipitate, which when dried contracts, and becomes of a pale
brownish-yellow colour: it dissolves readily in cyanide of po-
tassium, with the assistance of a little heat, or by digestion,
and constitutes the salt in question. 3. It may also be ob-
tained, as already mentioned, by adding a solution of cyanide
of potassium to terchloride of gold until the solution becomes
colourless. As the reactions which take place with these so-
lutions are exceedingly interesting, and calculated to add evi-
dence to the existence of a hitherto unobserved law, which
will be fully brought out when treating of the double silver

salt, and to explain similar reactions with platina and other metals, we will endeavour to follow them.

13. When a saturated solution of cyanide of potassium is added, in small portions, to a cold and strong solution of nitromuriate of gold, there is at first a violent effervescence, the solution becomes slightly turbid, and then quite transparent. The reaction may be explained as follows :—

The first effect is the neutralization of the excess of nitric and muriatic acids contained in the nitro-muriate of gold. This results at the expense of the cyanate of potash and cyanide of potassium.

Thus—$2KCy + KO, CyO + 2HCl + NO_5 + 3HO$, producing

$2KCl + KONO_5 + 2CO_2 + NH_3 + 2CyH$,

or represented thus :

$$2KCy \begin{cases} 2Cy \\ 2K \end{cases} \begin{matrix} \\ \end{matrix} 2CyH$$
$$2HCl \begin{cases} 2H \\ 2Cl \end{cases} \begin{matrix} \\ \end{matrix} 2KCl$$

and forming—

$$KO + CyO \begin{cases} 3H \\ 3O \\ N \\ 2C \\ O \\ KO \end{cases}$$

$$3HO \qquad NH_3$$
$$2CO_2$$
$$NO_5 \underline{\hspace{3cm}} KO + NO_5$$
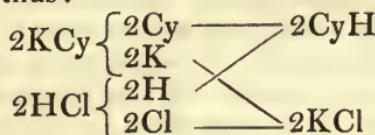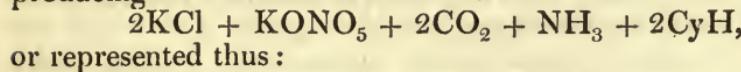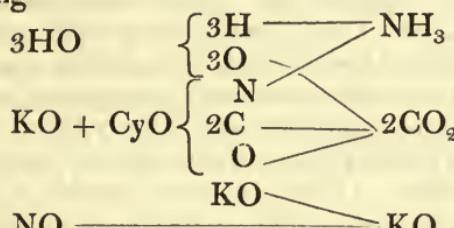
The solution is now free from acid; and on a further addition of cyanide of potassium it immediately, by the assistance of water, reacts upon the terchloride of gold. This will be evident from the following formulæ,

$$2AuCl_3 + 8KCy + 8HO,$$

producing

$2(AuCy + KCy) + 6KCl + 2(NH_3 + CO_2) + 2HCy$,

or it may be represented thus :—

$$2AuCl_3 \begin{cases} 2Au \\ 6Cl \end{cases} 2(AuCy + KCy)$$
$$\begin{cases} 2KCy \\ 6KCy \begin{cases} 6K \\ 2Cy \\ 2Cy \begin{cases} 4C \\ 2N \end{cases} 4CO_2 \\ 2NH_3 \end{cases} 2(CO_2 + NH_3) \\ 2Cy \end{cases}$$
$$8HO \begin{cases} 8O \\ 8H \begin{cases} 6H \\ 2H \end{cases} 2HCy \end{cases}$$
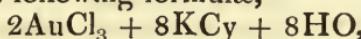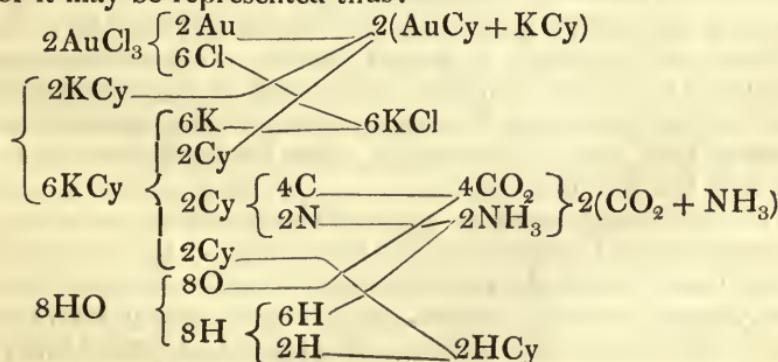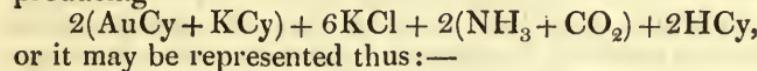
When, however, the solution of nitro-muriate of gold is re-
cently prepared, saturated and hot, a portion of the ammonia
formed reacts upon a portion of the gold, forming aurate of
ammonia, which precipitates along with the cyanide of gold;
on the addition of cyanide of potassium the ammonia is libe-
rated and the double salt of gold and potassium is produced.

14. The following process which we have employed is
beautifully calculated for obtaining a large quantity of salt in
a very few hours, and at comparatively little trouble.

A solution of cyanide of potassium is prepared, which con-
tains about six times as much cyanide as the quantity of gold
wished to be dissolved. The cyanide may be dissolved in
twice, thrice or four times its weight of distilled water and care-
fully filtered to separate any insoluble matters with which the
cyanide is usually contaminated; this solution is then to be
gently and carefully raised to a temperature of 100° F. in an
earthenware or glass vessel. Two plates of gold are placed
in the solution at the opposite sides of the vessel, and these
connected by means of copper wires to the poles of a small
galvanic battery, composed of two or three pairs of zinc
and copper plates, and excited with dilute acid. As soon as
the circuit is completed the operation commences, gold is dis-
solved from that plate connected with the copper end of the
battery, passes into the solution, and is only partly deposited
upon the opposite plate, whilst a gentle flow of gas is evolved
from the negative pole. The solution of cyanide of potassium
rapidly consumes the gold which is dissolved at the positive
pole and becomes a double salt of cyanide of gold and potas-
sium. The gold which deposits upon the negative pole is in
minute grains, occasionally quite brown, and is beautifully
crystalline. The operation is much facilitated by having the
positive pole or plate of gold several times larger than the
opposite plate. At first the amount of gold deposited is very
small compared with that dissolved from the positive pole,
the solution retaining the greater proportion; but after some
hours, as the solution becomes saturated, the amount deposited
increases until there is a perfect balance. The solution is
now to be filtered and carefully evaporated; it yields an abun-
dant crop of crystals of the double salt. In this way half an
ounce of gold may be dissolved in a few hours, and nearly an
ounce of the salt obtained.

15. The salt obtained by either of these processes presents
the same general appearance and constitution. It crystallizes
in very minute octahedrons, which cluster together so as to form
long, jagged, irregular prisms, which again group round a
centre and form starlike masses; they possess a peculiar bitter

and metallic taste. They are perfectly colourless and transparent, but seem opake from being composed of numerous small crystals. They are soluble in four times their weight of cold water, and in less than their own weight of boiling water, in the proportion of 5 to 4. They are very sparingly dissolved by alcohol, and their solubility is not increased by boiling. When alcohol is added to a warm saturated solution of the crystals, the salt falls in fine white brilliant shining scales, which are quite opake. Their solution does not stain the hands by the action of light, neither is the solution affected by exposure to light. When nitric, muriatic, or sulphuric acid is poured over the crystals, they are not affected at first, but by standing some time they are quite decomposed, and cyanide of gold results. The cyanide of gold occasionally precipitates in minute shining crystals from the nitric solution. Their solution is not immediately effected in the cold by these acids, but a boiling temperature decomposes them, the yellow cyanide of gold precipitating. Tartaric, oxalic and acetic acids effect the same decomposition when boiled. The nitric, muriatic and sulphuric acids added to a cold saturated solution precipitate the salt undecomposed. The following metallic solutions were added to a solution of the salt, and the results were as follows:—

Nitrate of copper . . a white precipitate of the mixed cyanides.
Sulphate of zinc . . .   .
Nitrate of silver . . .   .
Nitrate of mercury . a yellow-white precipitate   ...
Acetate of lead . . . a white precipitate   ...
Protosulph. of iron . a white precipitate of the mixed cyanides,
              which changes to a purplish-blue on
              the addition of nitric acid.
Muriate of man- . small crystals appear on the sides of the
    ganese . . . . . .   glass on standing.
Muriate of tin . . . . a slight opacity, increasing to a precipitate on standing.

When exposed to a red heat with their own weight of carbonate of potash, the whole of the gold is obtained in the metallic state as a bright button. The carefully dried salt was treated in this way and the following results obtained:—

16. A salt prepared with oxide of gold and cyanide of potassium yielded the following results:—

        7 grs. yielded   4·67 gold = 66·71 per cent.
        5 grs.   ...   3·33 ... = 66·66   ...
       10 grs.   ...   6·63 ... = 66·30   ...

The salt prepared by the battery process was also examined; it gave the following results:—

20 grs. yielded 13·32 gold = 66·60 per cent.
20 grs.   ...   13·25 ... = 66·25   ...

A sample of the former salt was carefully recrystallized, and placed for some time under a receiver over oil of vitriol; it gave the following result:—

20 grs. yielded 13·35 of gold = 66·75 per cent.

The mean of these results is . . . . . 66·54   ...

10 grs. of the salt were treated with nitric acid, and, the acid being completely expelled by evaporation, they were carefully collected and dried, yielded 8·85 grs. of cyanide of gold = 88·50 per cent.

The calculated amount of gold and of cyanide of gold contained in a salt consisting of single equivalents of cyanide of gold, cyanide of potassium and water, is of gold 66·37, and of cyanide of gold 88·30.

Our results approximate so closely to the calculated result, that we consider ourselves warranted in giving the formula

$$AuCy + KCy + HO.$$

17. In conclusion we may add, that from these results we have no hesitation in saying, that the salt described by Meillet as prepared by saturating chloride of gold with cyanide of potassium is not the auro-cyanide of potassium, but the cyanide of gold and potassium as described above.

IX. *On the Cyanides of the Metals, and their Combinations with Cyanide of Potassium.* Part II. *Cyanide of Silver.* *By* Messrs. CHARLES F. O. GLASSFORD *and* JAMES NAPIER[*].

THE compound that is formed by the union of cyanogen with silver has been long known, and its composition ascertained to be 1 equivalent of silver with 1 equivalent of cyanogen. The method of preparing this salt, as given in most chemical works, is by adding hydrocyanic acid to the nitrate or any other soluble salt of silver, when the cyanide of silver falls as a white precipitate. This method we have found, on the large scale, to be both disagreeable and tedious, and have therefore adopted the following method for its preparation. To a neutral solution of nitrate of silver is added cyanide of potassium so long as a precipitate is formed. No hydrocyanic acid is given off, the precipitate being washed from the nitrate of potash may be used for any purpose required. If the cyanide of potassium, however, contains any cyanate or carbonate, they form impurities in the precipitated cyanide of silver; these can be separated by digesting the

precipitate for a short time in dilute nitric acid: but if the cya-
nide of potassium contains any chlorides or undecomposed
ferrocyanide, these impurities cannot be separated by this
means. The best method of obtaining cyanide of silver free
from all these impurities, is to add a solution of nitrate of sil-
ver to a solution of the crystals of cyanide of potassium and
silver. Pure cyanide of silver precipitates, although the cya-
nide of potassium originally used contained all the impurities
before enumerated.

The cyanide of silver should not be dried at a heat exceed-
ing 260°, as it assumes a brownish colour when exposed to a
higher temperature. It also becomes brown by exposure to
light.

Hydrochloric acid converts the cyanide of silver into the
chloride with the rapid evolution of hydrocyanic acid.

Nitric acid has no effect upon it except when concentrated
and boiling.

Sulphuric acid diluted with its own volume of water de-
composes the cyanide of silver when boiling, with the escape
of hydrocyanic acid, and the formation of sulphate of silver,
which crystallizes by cooling, or slight dilution with water;
by this means the cyanide may be separated from the chloride,
which is not soluble in sulphuric acid.

Cyanide of silver is soluble in the alkaline chlorides, and in
the chlorides of calcium barium and magnesium when boiled,
though it is but slightly soluble in these latter when cold. It
is also soluble in ferrocyanide of potassium, forming one or
two distinct salts easily crystallized, but which we have not
yet sufficiently investigated to warrant a more particular notice
of them at present. It dissolves freely in hyposulphite of soda,
forming a crystalline compound by evaporation.

The best solvent of cyanide of silver is cyanide of potassium,
requiring only one equivalent of the latter to one equivalent of
the former, and constituting a distinct compound. This solu-
tion, when evaporated, yields an abundant crop of crystals of
a mixed character, most generally in the form of hexagonal
plates, somewhat resembling chlorate of potash, often inter-
mixed with numerous small crystals, chiefly rhombic prisms,
more transparent than the former crystals, which transparency
they lose by drying. Their proportion to the other crystals
is generally very small. Many crops of crystals are ob-
tained without one of these rhombs being perceptible. Their
proportion varies according to the rapidity or slowness of
evaporation and variation in the preparation of the salt.

These two forms of crystals were carefully separated and
dried at a temperature of 220°. A higher temperature causes

the crystals to assume a brownish colour, rendering them exceedingly friable; this condition was therefore cautiously avoided. They were then submitted to the following analysis:—25 grains were mixed with an equal weight of carbonate of potash and fused; the silver obtained in three different experiments was as follows:—

	I.	II.	III.	Mean
25 grs. of the rhombic prisms gave of silver	12·81	12·88	12·91	12·87
25 grs. of the hexagonal plates ...	13·5	13·38	13·43	13·43

The cyanogen contained in these salts was determined as follows: 25 grains of the crystals were dissolved and saturated with nitrate of silver; the precipitate, washed, dried, and afterwards fused with carbonate of potash, gave of silver:—

	I.	II.	III.	
From the rhombic prisms . . .	25·80	25·55	25·7	Mean 25·68
From the hexagonal crystals . .	26·55	26·63	26·65	Mean 26·61

The mean of these results corresponds with 26·00 of cyanogen in the hexagonal crystals, and 25·08 in the rhombs.

The amount of potassium was not determined by experiment, but calculated from the above results; but from the fact that a solution of the crystals had not the slightest effect upon test papers, proves the absence of free potash; and the precipitate formed by the addition of nitrate of silver to a solution of the salt not being effected by nitric acid, proved that all the potassium in the salt existed as a cyanide.

The results of these analyses in the 100 parts are—

Hexagonal Plates
Silver .	53·72
Cyanogen	26·00
Potassium	19·28
	99·00

Calculated without water.
Silver .	54·02
Cyanogen	26·40
Potassium	19·58
	100·00

Rhombs.
Silver .	51·48
Cyanogen	25·08
Potassium	18·59
	95·15

Calculated with water.
Silver .	51·69
Cyanogen	25·27
Potassium	18·70
Water .	4·34
	100·00

The water in these crystals was not ascertained by experiment, as it seems to be retained up to the point of decomposition, when cyanogen is also evolved; but these results correspond so closely with those calculated, that we have no hesitation in giving the formula of the one as $CyAg + CyK$, and the other as $CyAg + CyK + HO$.

These crystals possess similar properties in every other respect, and may afterwards be spoken of as one salt. They are soluble in 8 parts of cold and in an equal weight of boiling water; are soluble in boiling alcohol, but are deposited again

by cooling; they do not deliquesce in the air; their solution does not affect test papers, has a sweet taste when first taken into the mouth, but becomes disagreeably bitter, and the sensation is not easily removed; it may be boiled for any length of time without decomposition.

The crystals are easily affected by light, the solution very little so; but a piece of paper dipped into it and dried is very sensitive to light, and would, in our opinion, be a superior salt for photogenic processes to any at present in use. We may also observe here that the analogous salt of gold is excellently adapted for the same purposes.

Cyanide of potassium and silver is decomposed by all acids, precipitating the silver as a cyanide; the hydracids however decompose the cyanide of silver also.

Sulphuretted hydrogen precipitates the silver from solutions of cyanide of potassium and silver as a sulphuret. A solution of the salt is not effected by alkaline or earthy chlorides, nor by carbonates.

Perchloride of mercury gives a white   precipitate.
Protosalts of iron . . . . a brownish-white   ...
Persalts of iron . . . . a brownish-yellow   ...
Lead, nitrate and acetate . . . a white   ...
Protosalts of tin . . . . a yellowish-white   ...
Copper salts . . . . a light green   ...
Sulphate of zinc . . . . a white   ...
Gold . . . . a yellowish-white   ...
Nitrate and chloride of cobalt . a pink, passing to a light violet.
Manganese sulphate . . . . . no precipitate.

In the last paper we referred to the superior affinity of cyanogen for silver in the presence of cyanide of potassium, decomposing every salt of silver but the sulphuret, to form cyanide of potassium and silver. As, for example, if cyanide of potassium is added to cyanate of silver, the latter is decomposed by 2 equivalents of cyanide of potassium, and there is produced one of cyanide of potassium and silver with one of cyanate of potash. The reaction may be expressed thus:
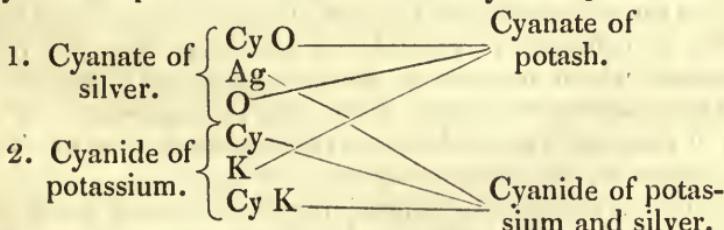
A similar reaction takes place when cyanide of potassium is added to the oxide of silver, carbonate of silver or chlo-

ride of silver. It is mentioned in Prof. Graham's 'Elements of Chemistry,' upon the authority of Prof. Liebig, that the chloride of silver is soluble in cyanide of potassium, and that the solution yields a double salt by evaporation. We have repeatedly tried to obtain this double salt, but cannot say we have been successful; it requires two equivalents of cyanide of potassium to obtain the chloride apparently in solution, which then contains chloride of potassium and cyanide of potassium and silver: by repeated crystallization and taking the first crop, we get the latter salt free from the former, which was determined by dissolving some of the crystals thus purified in water and saturating the solution with nitrate of silver. The precipitate being afterwards boiled in sulphuric acid, as noticed in a former part of the present paper, the whole precipitate was dissolved, which would not have occurred had any chloride been present.

It is upon this superior power of silver to combine with cyanide of potassium to form the double salt just described, that we have adopted the method of determining the per-cent-age of cyanide of potassium in any sample. Nitrate of silver is dissolved in distilled water to such a strength, that two gra-duations of a common alkalimeter will contain exactly 1 grain of nitrate of silver; 25 grains of the sample to be tried are dissolved, and this solution of nitrate of silver is added cau-tiously until the precipitate formed ceases to be redissolved; the number of graduations of the silver solution taken are noted, and calculated according to the following equation:—as 170 is to 65·5 so is the number of graduations taken, to the answer, which must be multiplied by 4 to obtain the per-centage. Ni-trate of silver of any strength may be used without a graduated measure, but the solution must in that case be evaporated to dryness, treated with hydrochloric acid, again evaporated to dryness, and fused to know the quantity of silver taken; the amount of cyanide of potassium may be known by the following calculation:—As 108·0 is to 131·08 (2 equivalents of cyanide of potassium), so is the weight of silver obtained multiplied by 4 to the per-centage of cyanide of potassium in the sample.

The substitution of cyanide of potassium for any other substance which may be in union with silver is beautifully shown by adding cyanide of potassium to ferrocyanide of sil-ver: it requires 4 equivalents of the former to 1 of the latter to produce 2 equivalents of cyanide of potassium and silver, and 1 of ferrocyanide of potassium; the solution being eva-porated the two salts crystallize distinct, and may be easily separated both by their shape and colour. In some expe-riments of this kind we have been able to recover within one

per cent. of the whole of the ferrocyanide of potassium originally used in precipitating the silver.

When cyanide of potassium is added to the ferridcyanide of silver a similar substitution takes place, requiring 6 equivalents of cyanide of potassium to get the whole into solution; but the primary action is somewhat different in this salt from any other we have tried. When cyanide of potassium is added to ferrocyanide of silver, the formation of the double salt of silver and potassium, and the ferrocyanide of potassium, are simultaneous; and if there be not 4 equivalents of cyanide of potassium added, the remaining precipitate is undecomposed ferrocyanide of silver. But on adding cyanide of potassium to the ferridcyanide of silver, the first action is the decomposition of the whole ferridcyanide of silver, with the formation of cyanide of silver and ferridcyanide of potassium; 3 equivalents of cyanide of potassium effect this change, the precipitates passing from an orange to a white; the ferridcyanide of potassium may be decanted from the precipitate without having any silver in solution. Several other interesting reactions take place with these substances, which have not yet been sufficiently investigated to be brought forward. In conclusion, however, we may mention a reaction with the ferrocyanides of the metals, which, so far as we are aware, has not yet been observed in any chemical work; it is that the ferrocyanides of the metals are converted into the ferridcyanides by adding an excess of nitric acid to these compounds; if, for example, we add nitric acid to the ferrocyanide of silver, it is immediately changed from a white to a deep orange; 1 equivalent of silver is dissolved, and every 2 equivalents of ferrocyanide of silver with nitric acid produces one of nitrate silver and one of ferridcyanide of silver.

When the ferridcyanide of silver is heated in water to about 150°, it loses its orange colour and becomes green, which colour is permanent; any acid or nitrate of silver present prevents this change until the precipitate is dried and heated. This green precipitate has not yet been examined. It can be obtained either with the ferridcyanide formed by the nitric acid upon ferrocyanide, or by precipitating silver with ferridcyanide of potassium.

X. *Proceedings of Learned Societies.*

ROYAL ASTRONOMICAL SOCIETY.

[Continued from vol. xxiv. p. 540.]

March 8, 1844. THE following communications were read:—I. Observations of the Comet of Faye*.

[* Other observations of this comet will be found referred to in vol. xxiv. pp. 519—522.]

1. Observations made at Starfield. By W. Lassell, Esq., for which we refer to the Monthly Notices of the Society, vol. vi. No. 5.

2. Right Ascensions and North Polar Distances of the Comet of Faye, from Observations at the Royal Observatory, Greenwich. Communicated by G. B. Airy, Esq., Astronomer Royal.

Day of observation.	Mean solar time for R.A.			True right ascension of comet.			No. of obs. in R.A.	Mean solar time for N.P.D.			True N.P.D. of comet.			No. of obs. in N.P.D.	Remarks.
1843.	h	m	s	h	m	s		h	m	s	°	′	″		
Nov. 29	12	30	46	5	21	36·05	14	12	38	40	84	25	36·0	11	Meridian observation with the mural circle.
		......			......		...	12	48	37	84	25	32·1	1	
	17	33	9	5	21	28·19	6	17	33	9	84	28	9·9	6	
Dec. 11	9	1	7	5	15	59·50	7	9	1	7	86	8	25·2	7	
15	14	4	37	5	13	58·87	7	14	28	14	86	31	40·1	1	
1844.															
Jan. 15	9	55	30	5	9	24·43	2	9	54	10	86	9	(32·6)	1	Meridian observation with transit instrument and mural circle.
22	8	17	27	5	11	55·62	6	8	16	29	85	33	5·1	5	
26	11	29	0	5	14	(4·00)	2	11	29	0	85	7	(19·0)	2	
Feb. 8	8	11	30	5	23	39·71	1	8	11	30	83	42	24·7	1	
19	11	46	9	5	34	45·58	2	11	46	9	82	26	38·9	2	
20	8	41	32	5	35	46·15	6	8	44	20	82	20	26·8	5	
22	8	18	13	5	38	2·19	10	8	12	26	82	8	1·1	6	

The above observations, with the exceptions mentioned in the remarks, were made with the south and the east equatoreals of the Observatory, the observations of Nov. 29 and Dec. 11 being made with the latter and all the rest with the former instrument. In general, the observations consisted of comparisons, in the same position of the polar axis of the instrument, of the comet with neighbouring stars in both elements, the differences of right ascension of the star of comparison and the comet being determined by the time of their transits across the declination-wire, and the difference of north polar distances by the reading of the declination circle of the east equatoreal, and of the sector-arc of the south equatoreal. The only exceptions to this rule are the results of Jan. 15 and Jan. 26, 1844 (included in brackets), which depend on the readings of the hour-circle and declination-circle of the south equatoreal, and which may be affected with errors of 3ˢ or 4ˢ in right ascension, and of 1′ in north polar distance.

The results have been rigorously cleared of the effects of refraction and parallax, the distances of the comet from the earth being taken from an ephemeris, by Professor Henderson, in the Monthly Notice of this Society for January last, as far as it extends.

The places of the stars of comparison have been taken either from the Catalogue of Santini, contained in vol. xii. of the Memoirs of this Society, or from meridian observations made since at the Royal Observatory. The only element which is at all doubtful is the right ascension of the star of comparison for the observations of Feb. 20, which has been deduced from the time of its transit across the central wire of the telescope of the mural circle. It is believed that this determination is not in error to the amount of 1ˢ, and the result may

be used without scruple at this stage of the correction of the orbit. On Feb. 19, 20, and 22, the comet was excessively faint, and it required a great deal of caution to observe it. The single results are, however, for those evenings, on the whole very consistent, the north polar distances observed on Feb. 22 being subject to greater uncertainty than the rest. It was extremely difficult to bring the comet to the equatoreal wire of the instrument on that evening, though the times of transit on the same evening were observed with tolerable accuracy.

3. Right Ascensions and Declinations of the Comet of Faye, observed with the Equatoreal at the Observatory of Trinity College, Dublin, 1844. By Mr. Charles Thompson, Assistant. Communicated by Sir W. Hamilton.

4. Observations of the Comet of Faye. By C. Rumker, Esq. Communicated by Dr. Lee.

5. Observations of the Comet of Faye made at the Observatory, Durham; lat. North $54° 46' 6''$; long. West $6^m 18^s$. Communicated by the Rev. Temple Chevallier.

6. Observations made at Hartwell, Bucks. By the Rev. J. B. Reade. Communicated by Dr. Lee.

The substance of the preceding four communications will be found in the Monthly Notices, vol. vi. No. 5.

XI. *Intelligence and Miscellaneous Articles.*

ON THE LAWS WHICH GOVERN THE ELECTRO-CHEMICAL DECOMPOSITION OF BODIES. BY M. EDMOND BECQUEREL.

THE object which I proposed to myself in the essay which I have the honour to submit to the Academy, is to make known the laws which govern the electro-chemical decomposition of bodies.

Mr. Faraday laid down as a principle, that when the same electric current was made to pass through several metallic solutions, such as those of nitrate of copper, of lead, of silver, &c., equivalent quantities of metal were deposited at the negative pole in each solution. But this law is true only in a small number of cases, when the salt is formed of an equivalent of oxide and an equivalent of acid, and does not account for the effects observed in the decomposition of a great number of combinations.

As an example, I will select the hyponitrates of lead, which deposit at the negative pole two and three times and a half as much lead as the nitrate for the same current.

I have examined the decomposing action of electricity on series of well-analysed bodies; I thus successively submitted to experiment all the metallic chlorides, the oxides and oxygenated water, the acetates and the hyponitrates of lead, &c.

All the results which I obtained have led me to the following conclusions :—

When a binary or ternary compound is submitted to the decomposing action of electricity, the decomposition always takes place in

a definite proportion, so that, for one equivalent of electricity employed, one equivalent of the electro-negative element, or at least of the compound which acts the part of acid in the combination, is carried to the positive pole, and the corresponding quantity of the electro-positive element, or that which serves as base, is carried to the negative pole.

This law may be expressed as a formula in the following manner:—

One equivalent of a combination formed by the union of an equivalent of acid and of a corresponding quantity of base, always requires one equivalent of electricity for its electro-chemical decomposition.

I have given the name *equivalent of electricity* to the quantity of electricity necessary to decompose one equivalent of water.

These laws are relative only to the direct effect of the current, and not at all to the secondary effects; for there are some salts, such as the acetates of lead, which are only decomposed by the reducing action of the hydrogen arising from the decomposition of the water, and which always give an equivalent of metal to the negative pole.

If an equivalent of electricity be required to decompose an equivalent of any combination whatever, it may be admitted that if the two elements, electro-positive and electro-negative, which form the combination, be separated and again combine, they disengage exactly one equivalent of electricity. Hence, by referring to the law stated above, the following conclusions, which are *very important as regards molecular chemistry*, are deduced:—

1. When an equivalent of a body, whether simple or compound, combines with one or several equivalents of another body, if the first acts the part of an acid in the combination, the liberation of electricity which results from their chemical action is such, that an equivalent of electricity is always produced.

2. If an equivalent of a body, such as oxygen, has already entered into combination with another which acts as a base, and if the combination again unites with an equivalent of the first body, that is to say, of oxygen, to form a deuto salt, at the time of this second action another equivalent of electricity is disengaged.

Thus the quantity of electricity set free depends solely on the body which acts the part of acid in the combination.

My memoir only treats of inorganic substances, because the organic compounds are generally non-conductors of electricity, and, when they conduct, the secondary effects so conceal the direct effect, that it is very difficult to recognise the definite action of the electricity.

The principles which I have laid down very well represent all the results obtained with regard to the direct effects, so that there is a constant relation between the chemical theory of equivalents and the decompositions by electricity. Their establishment was of the greatest importance, for they should serve as the point of departure for every electro-chemical theory.—*Comptes Rendus*, March 4, 1844.

PROTO- AND PER-ARSENITE AND ARSENIATE OF IRON.

M. Damour prepared these compounds by precipitating, according

to circumstances, the proto- and perchlorides of iron with excess of arsenite or arseniate of potash.

Protoarsenite of Iron.—This is immediately precipitated in whitish flocks; but by washing and the contact of the air, it soon becomes of an ochre-yellow colour. Dried in this state and heated in a tube, it yields water, and afterwards fuses, swells up and produces a white crystalline sublimate of arsenious acid. The fused mass is of a deep rust colour.

When treated with potash it becomes of a very black colour, which is to be attributed to the black magnetic oxide resulting from the partial oxidizement of the protoxide of iron; by continued exposure to the air while moist, it became of the brown colour of hydrated peroxide of iron.

Perarsenite of Iron.—This was of a fine ochre-yellow colour, which did not alter during washing carried on in contact with the air. Dried and heated in a glass tube it swelled up and fused, yielding at first water and afterwards a white sublimate of arsenious acid. When treated with potash the greater portion of it was dissolved; the solution, as well as the undissolved portion, were of the colour of rust of iron.

Protoarseniate of Iron.—Recently precipitated it is of a white colour, but becomes bluish-gray by exposure to the air. Dried at a moderate heat and then heated to redness in a tube it fused, yielding water and a white sublimate, which was not crystalline, but destroyed the transparency of the glass; the fused mass became of a blackish colour. The protoarseniate, which had partly become arseniate of the black oxide of iron, by exposure to the air, was mixed with potash and became immediately of a black colour; this, by some days' exposure to the air while moist, became of the brown colour of hydrated peroxide of iron.

Protoarseniate of iron dissolved in cold hydrochloric acid, and treated with a few drops of a solution of chloride of sodium and gold, soon occasioned the production of a considerable quantity of a brown powder, which, when exposed to the heat of the blowpipe, fused into a brilliant globule.

Perarseniate of Iron was white with a tint of gray; washed in contact with the air its appearance did not alter. After drying at a moderate temperature it was heated to redness in a tube, and yielded water without undergoing fusion, and without producing any sublimate. Put into contact with potash it became immediately of a rust-brown colour, without any black tint.—*Ann. de Ch. et de Phys.*, Avril 1844.

DESCRIPTION AND ANALYSIS OF PENNINE.   BY MM. MARIGNAC AND DESCLOIZEAUX.

This substance, discovered and named by M. Fröbel of Zurich, occurs in crystals or large lamellar crystalline masses.

The primary form is an acute rhomboid of 63° 15′. The very small crystals only possess this form perfectly; the larger ones are very deeply truncated in a direction perpendicular to the axis, so that they become tables of less or greater thickness, with triangular or hexagonal bases, the planes of which are triangles or trapeziums, forming

alternately with the base an acute angle of 79° 30', and an obtuse supplementary angle. This substance cleaves readily into very small laminæ perpendicular to the axis; when these are not too thick they may be broken in the direction of the three sides of an equilateral triangle, parallel to three faces of the rhomboid.

The hardness on the bases rather exceeds that of gypsum, and on the faces of the rhomboid it is nearly equal to that of carbonate of lime; the thin laminæ are very ductile, but not elastic; the powder, which is light greenish-white, is greasy to the feel.

The specific gravity varies from 2·653 to 2·659; this difference appears to be owing to the presence, in some of the crystals, of octahedral iron.

When heated to redness in a tube, water is obtained; it splits into leaves before the blowpipe, becomes white and fuses with difficulty into a gray enamel. It dissolves in the salt of phosphorus, or leaves a skeleton of silica; the glass, coloured by the iron while hot, becomes opaline on cooling. With soda on a strip of platina a slight yellow colour is produced.

When in fine powder it is perfectly acted upon by long boiling in hydrochloric acid.

Two very pure crystals from the valley of Zermatt in the Valais, and a third in foliated crystalline masses from the valley of Binnen (Valais), gave by analysis the following results:—

	I.	II.	III.
Silica	33·36	33·40	33·95
Alumina	13·24	13·41	13·46
Oxide of chromium . .	0·20	0·15	0·24
Peroxide of iron	5·93	5·73	6·12
Magnesia	34·21	34·57	33·71
Water	12·80	12·74	12·52
	99·74	100·	100·

Pennine is found in amianthiform chloritic slate, in the serpentine rocks near Monte Rosa. It was first observed in crystals having the appearance of long triangular prisms, which are merely ill-formed rhomboids, in a grayish-white schist in the valley of Binnen; it is the mineral which M. Necker in his *Traité de Mineralogie* calls *hydrotalc.—Ann. de Ch. et de Phys.*, Avril 1844.

ON THE PREPARATION AND PROPERTIES OF CERTAIN CHLORATES. BY M. ALEXANDER WAECHTER.

Chlorate of Lithia.—This compound was prepared by dissolving carbonate of lithia in solution of chloric acid. The neutral liquor was evaporated over sulphuric acid; it yielded no crystals of determinate form, but became a radiating crystalline mass. It was pressed between folds of blotting-paper, and then perfectly dried over sulphuric acid.

The chlorate of lithia thus procured is a white, deliquescent saline mass, which is very soluble in alcohol; it fuses at 122° Fahr., and begins to lose water, oxygen and a little chlorine at 284°; when heated till gas ceases to be evolved, there remains chloride of lithium

with alkaline reaction. To determine the composition of this chlorate it was first converted into a chloride by hydrochloric acid, and afterwards into sulphate by sulphuric acid. It is a neutral salt composed of

Chloric acid	76·31
Lithia	14·59
Water	9·10
	100·

Chlorate of Ammonia.—This was obtained by decomposing chlorate of barytes with carbonate of ammonia; the clear solution was evaporated over sulphuric acid, and yielded prismatic crystals, the form of which could not be accurately determined. This salt is very soluble in water, and but little so in absolute alcohol. Vauquelin states erroneously, that this salt is volatilized below the boiling point of water; when heated to 215° Fahr. it decomposes suddenly, exhibiting a red light. The products of the decomposition are the vapour of water, azote, chlorine, oxygen, hydrochloric acid and hydrochlorate of ammonia. When mixed with combustible substances, it detonates as the chlorate of potash does. In order to determine the composition of this salt it was first converted into chloride of potassium, which was decomposed by nitrate of silver. It was found to be composed of

Chloric acid	74·25
Ammonia	25·75
	100·

Chlorate of Barytes.—This salt emits light while crystallizing; its form is a rhombic prism; when heated to 248° it loses 5·88 per cent. of water, and begins to yield oxygen at 482°; at above 752° it fuses, and then yields all its oxygen with a trace of chlorine. If the heat be quickly raised, it decomposes suddenly with detonation; when mixed with combustible bodies it decomposes, like the chlorate of potash, by percussion. It is very sparingly soluble in absolute alcohol, and colours the flame of alcohol deep green.

It yielded by analysis,—

Chloric acid	47·59
Barytes	46·53
Water	5·88
	100·

Journ. de Pharm. et de Ch., Mai 1844.

ON THE CHLORIDES OF GOLD.

Great difficulty has hitherto occurred in preparing the chloride of gold, of the yellow and red colours, perfectly soluble in water and without suffering reduction. The following processes are recommended for this purpose:—

1st. In order to prepare the yellow salt of gold, take *aqua regia* prepared with three parts of hydrochloric acid, one part of nitric acid and one of distilled water. Then put one part of pure gold into a porcelain capsule and pour the *aqua regia* upon it; cover the capsule with a plate of glass and heat it in a salt water bath, the heat being continued till red vapours cease; the cover is then to be removed, and if the gold is not entirely dissolved, some *aqua regia* is to be

added to it; the capsule being again covered, the heat is to be continued till vapour ceases to appear; the glass plate must then be removed and replaced by folds of blotting-paper, the heat being continued in the bath, until a glass rod being immersed in the capsule, it becomes covered, on removing it, with yellow, solid chloride of gold.

The capsule is then to be removed from the salt-water bath, and the chloride of gold soon crystallizes in small prismatic crystals, of a fine yellow colour, with an orange tint. The chloride thus obtained is perfectly soluble in water without reduction; it is successfully employed in Daguerreotype and other operations.

The *red* chloride of gold (terchloride) is prepared in the same manner, except that the *aqua regia* employed is prepared with two parts of hydrochloric and one part of nitric acid. The operation is commenced by acting upon gold with excess of *aqua regia* on a sand bath, the salt water bath not being used until the gold is entirely dissolved; the remainder of the operation is conducted in the same manner as that for the yellow chloride.—*Journ. de Ph. et de Ch.*, Mai 1844.

ON BRESILIN AND BRESILEIN. BY M. PREISSER.

Bresilin is the colouring matter of Brazil, Fernambuc or Nicaragua wood; this colouring principle was discovered by M. Chevreul, who obtained it by treating Brazil wood with alcohol, in the same manner as he obtained hematin from logwood.

This process, however, M. Preisser states, yields only an impure product; he procured it by acting on the interior and slightly coloured part of Brazil wood, in a state of great purity.

The properties of bresilin are like those of hematoxylin, recently isolated by Erdmann; it exists in small colourless needles, which appear to be rectangular prisms. Its taste is sweet, with a slightly bitter after-taste. It is soluble in water, and the solution may be long kept without alteration; it becomes coloured only on the edges, of a lively red colour. By boiling the colouring takes place much more rapidly; the liquor becomes of a fine crimson-red colour, and if this red coloured liquid is evaporated, it deposits numerous satin-like crystals of a bright and very fine red colour.

M. Preisser gives the name of *bresilein* to bresilin coloured of a bright red. Bresilin is soluble in alcohol and in æther; with the contact of air it is coloured bright red by means of hydrochloric acid; sulphuric acid dissolves it, renders it yellow and soon blackens it. Dilute nitric acid reddens it very strongly; if the mixture be heated, red vapours are disengaged and oxalic acid is produced.

The action of chromic acid and the alkaline chromates is very remarkable, and it is the more important to insist upon this action, as it explains numerous important applications in calico printing.

As soon as chromic acid or bichromate of potash in fine powder is added to a concentrated solution of bresilin, brisk effervescence ensues, and by distillation the liquor yields a notable quantity of formic acid; the liquor becoming of a deep brown colour, and after some hours a deep crimson lake separates. This lake is formed of the modified

colouring matter (*bresilein*) and oxide of chromium; when it is washed with diluted hydrochloric acid, the colouring principle is dissolved, and pure green oxide of chromium is left.

When solution of ammonia is poured directly upon the crystals of bresilin, they become immediately of a deep purple-red colour; but without the contact of air little change is produced.

Potash and soda give with bresilin, in contact with air, the colour of venous blood, which hydrochloric acid precipitates in an hour. Lime water reddens the solution of bresilin; nitrate of silver and chloride of gold are reduced when boiled with bresilin, while acetate of lead gives a whitish-yellow precipitate, becoming brown by drying. When decomposed by heat no traces of ammonia are perceptible.

By analysis bresilin yielded,—

Carbon	66·36
Oxygen....	29·46
Hydrogen ..	4·18
	100·

and bresilein gave—

Carbon	63·07
Oxygen	32·97
Hydrogen ..	3·96
	100·

It will appear by calculation that the first of these substances is converted into the second by absorbing two equivalents of oxygen from the air.—*Journ. de Ph. et de Ch.*, Mars 1844.

METEOROLOGICAL OBSERVATIONS FOR MAY 1844.

Chiswick.—May 1. Dry haze: excessively dry: clear and fine. 2, 3. Cloudless: excessively dry. 4. Slight rain. 5—7. Overcast and fine. 8. Dry haze. 9. Sultry. 10. Overcast: very fine. 11, 12. Very fine. 13. Sultry. 14, 15. Very fine. 16. Cloudy and fine. 17. Cloudy and windy. 18, 19. Boisterous. 20. Boisterous: cold and dry. 21. Drizzly. 22. Dry haze: very fine. 23. Cold haze. 24. Cloudy and cold: fine. 25. Overcast: fine: clear. 26, 27. Cloudy and cold. 28. Cloudy. 29. Overcast: slight drizzle: rain at night. 30. Cloudy. 31. Overcast: fine: clear.—Mean temperature of the month 1°·2 below the average.

Boston.—May 1—3. Fine. 4. Cloudy. 5—7. Fine. 8, 9. Cloudy. 10. Cloudy: rain P.M. 11. Fine. 12. Cloudy. 13, 14. Fine. 15. Cloudy. 16. Fine. 17. Rain. 18. Cloudy: rain P.M., with rainbow. 19. Windy. 20, 21. Windy: rain P.M. 22, 23. Cloudy. 24. Cloudy: rain P.M. 25—31. Cloudy.

Sandwick Manse, Orkney.—May 1. Bright: clear. 2. Bright: cloudy. 3. Damp: clear. 4. Bright: clear. 5. Bright: cloudy. 6. Bright: rain. 7, 8. Bright: clear. 9. Damp: rain. 10. Drizzle: damp. 11, 12. Cloudy: clear. 13. Clear: showers. 14. Bright: cleared. 15. Cloudy. 16. Cloudy: showers. 17. Showers. 18. Clear: showers. 19. Bright: clear. 20. Bright: cloudy. 21, 22. Bright: damp. 23. Cloudy. 24. Cloudy: clear. 25. Bright: clear. 26, 27. Bright: cloudy. 28, 29. Cloudy. 30, 31. Cloudy: damp.

Applegarth Manse, Dumfries-shire.—May 1—3. Fine, but parching. 4—6. Very droughty. 7. One slight shower A.M. 8. Fair. 9, 10. Showers, slight. 11. Fair, but cloudy. 12, 13. Fair and clear. 14. Cool. 15—17. Fair and withering, 18. Hoar-frost A.M. 19, 20. Fair and very dry. 21. Very high wind. 22—28. Very withering. 29. Hoar-frost. 30. Hoar-frost: dry. 31. A few drops of rain.

Meteorological Observations made by Mr. Thompson at the Garden of the Horticultural Society at Chiswick, near London; by Mr. Veall, at Boston; by the Rev. W. Dunbar, at Applegarth Manse, Dumfries-shire; and by the Rev. C. Clouston, at Sandwick Manse, Orkney.

Days of Month 1844 May.	Barom. Chiswick Max.	Barom. Chiswick Min.	Barom. Boston 8½ a.m.	Barom. Dumfries 9 a.m.	Barom. Dumfries 9 p.m.	Barom. Orkney 9½ a.m.	Barom. Orkney 8½ p.m.	Therm. Chiswick Max.	Therm. Chiswick Min.	Therm. Boston 8½ a.m.	Therm. Dumfries Max.	Therm. Dumfries Min.	Therm. Orkney 9½ a.m.	Therm. Orkney 8½ p.m.	Wind Chiswick 1 p.m.	Wind Boston	Wind Dumfries	Wind Orkney	Rain Chiswick	Rain Boston	Rain Dumfries	Rain Orkney
1.	30·469	30·336	29·92	30·33	30·37	30·33	30·40	70	38	57	69	36	54	51	e.	calm	s.	w.				
2.	30·432	30·336	29·94	30·40	30·30	30·42	30·37	71	46	58	68	40½	56	52	e.	e.	s.	w.				
3. O	30·213	30·138	29·67	30·22	30·22	30·38	30·41	65	45	58	65½	38½	52	43	ne.	calm	sw.se.	w.	·01			
4.	30·132	30·090	29·72	30·29	30·21	30·40	30·35	59	48	51·5	58½	44	48	42	ne.	calm	ne.	e.				
5.	30·060	29·913	29·62	30·12	29·98	30·24	30·18	59	43	57	68	44	48	48	ne.	calm	ne.	s.				
6.	29·867	29·791	29·33	29·86	29·75	29·90	29·16	72	36	60	70	42	50	50	e.	calm	se.	se.				·10
7.	29·868	29·792	29·25	29·79	29·85	29·90	29·94	72	43	61	63	46½	54	46	n.	calm	nw.	w.				·50
8.	29·947	29·944	29·44	29·90	29·89	29·92	30·33	76	44	56	66	38½	50	54	e.	calm	s.	se.			0·02	
9. ☾	29·962	29·933	29·48	29·90	29·86	29·98	30·87	69	49	53	62½	34	50	51	sw.	calm	s.	s.	·07			·58
10.	29·942	29·910	29·34	29·84	29·91	30·00	30·30	78	38	56	55½	41	56	46	w.	w.	nw.	w.		·35		·10
11.	30·060	29·931	29·44	30·01	30·03	30·10	30·31	68	52	57	62	49½	48	48	ne.	calm	sw.	n.				
12.	30·262	30·183	29·64	30·10	30·21	30·10	30·29	64	41	56	66½	42½	52	46	ne.	calm	sw.	sw.				
13.	30·335	30·309	29·70	30·10	30·28	30·22	30·14	72	39	63	66½	43	53	47	ne.	calm	w.	sw.			0·26	·07
14.	30·322	30·272	29·73	30·29	30·31	30·33	30·37	78	38	60	64½	51½	53	47	ne.	calm	ene.	nw.				
15.	30·365	30·285	29·87	30·38	30·30	30·41	30·33	70	41	52	65	46	48	45	ne.	n.	ene.	w.				
16.	30·242	29·961	29·77	30·00	29·94	30·12	30·06	60	40	55	63	37	48	46	ne.	calm	sw.	sw.				
17. ●	29·964	29·886	29·40	30·00	29·98	30·05	30·06	65	32	49·5	53	41	49	36	ne.	ne.	n.	nw.	·02	·07		·10
18.	29·989	29·927	29·49	30·01	30·13	30·02	30·24	59	44	47	51	31½	38	40	ne.	n.	ne.nw	n.		·04		·15
19.	29·997	29·956	29·64	30·17	30·15	30·00	30·30	54	43	51	54½	34	43	43	ne.	e.	nne.	n.				
20.	29·988	29·982	29·62	30·13	30·08	30·00	30·26	60	40	51	52½	35½	46	45	ne.	n.	n.	n.				
21.	29·926	29·858	29·43	30·05	30·22	30·33	30·48	63	46	52	61	42	50	50	ne.	e.	ne.	w.	·02	·03		
22.	30·164	30·146	29·66	30·25	30·29	30·41	30·40	67	46	53	65	47	52	45	ne.	ne.	nne.	w.		·02		
23.	30·136	30·086	29·70	30·24	30·16	30·40	30·34	70	43	53	69½	50	48	46	ne.	n.	ne.	n.				
24.	30·076	29·968	29·55	30·14	30·08	30·25	30·22	66	45	54	66	44½	50	43	ne.	ne.	ne.	w.				
25. ☽	30·103	30·019	29·58	30·27	30·20	30·26	30·30	65	40	51·5	62	44½	49	47	ne.	ne.	ne.	nw.				
26.	30·206	30·156	29·70	30·27	30·30	30·35	30·34	58	41	51·5	60	38½	47	46	ne.	ne.	ne.	nw.	·06	·01		
27.	30·199	30·125	29·81	30·18	30·25	30·40	30·36	56	43	51	61	39½	48	46	ne.	ne.	ne.	nw.				
28.	30·026	29·956	29·73	30·16	30·08	30·36	30·22	59	48	53	61	43	50	47	e.	ne.	ne.	w.			0·00	
29.	29·935	29·927	29·55	30·03	30·04	30·20	30·20	58	44	52	59	45	48	45	ne.	ne.	ene.	ne.	·07			
30.	30·023	30·002	29·55	30·08	30·05	30·16	30·18	61	45	52	58½	41½	50	46	ne.	ne.	ene.	ne.			0·00	
31. O	30·035	30·001	29·54		30·03	30·20	30·15	61	42	52·5	60		50		e.	calm	ene.	nw.				
Mean.	30·104	30·036	29·69	30·103	30·111	30·207	30·220	65·10	42·68	54·3	62·1	42·1	49·67	46·22					0·25	0·52	0·28	1·60

XII. *Desultory Remarks on Academic and Non-Academic Mathematics and Mathematicians.* By J. J.*

THE general utility of the mathematics as a mental discipline will not perhaps be disputed. The study which consists of a constant reference to fundamental principles, generates habits of strict reasoning and fortifies the mind against casuistry and plausible assertion. Dugald Stewart ascribes peculiar and supereminent advantages to mathematicians, in consequence of the definite phraseology that they employ.

The benefits which have resulted to mankind from the cultivation of the mathematics cannot be denied. All our national greatness that rests on commerce, arts or arms, is based on mathematics actually applied to the concerns of life. However, the usefulness of mathematical learning appears to be generally admitted: at our principal seminaries it receives becoming attention. Oxford has lately made it a subject by which her favourite sons may gain distinction. At Cambridge it leads to the highest honours which that University has to confer. Cambridge has long been justly celebrated for its peculiar favour to mathematics, and the number of eminent mathematicians that it has produced prove that that seat of learning deserves the celebrity so generally ascribed to it. Those who have been so fortunate as to be members of that University know, and those who only attentively consider the published proceedings of the examinations, &c. may easily convince themselves, that Cambridge does not lag in the progress of mathematical science; if it be cultivated in other quarters, there it is forced on by every conceivable stimulant. To become all that Cambridge can make a student in three years— to become one of her choicest specimens in mathematical ac-

* Communicated by the Author.

quirements in that limited period—the student should have a private tutor. If, however, an undergraduate have not the advantage of a private tutor, by simply attending the college lectures during the time of his *statu pupillari*, there is scarcely a branch of physical science to which mathematics is applied, but will form the subject of lectures, or incidentally become a part of the course: he may begin with common arithmetic and end with the most refined and subtle analysis applied to the intricate laws of matter; by attending even the common routine of education pursued there, quite open to all, the student will be taken through nearly the whole round of the physical sciences under the guidance of competent directors, quite willing to assist and expedite his progress; consequently, if he have the capacity, as well as the wish, to follow up the track marked out in the lecture-room, he will, at the end of his undergraduateship, be intimately acquainted with all the mathematical sciences. It is not my business to speak of the complete course of classical and other education which the colleges supply, and to which they require and enforce due attention.

Some cavillers, who have never been at the University, and who very likely would not have much distinguished themselves if they had, sneer at the course of education pursued at Cambridge as mere trifling. There is also another class of critics; men who have been at the University, wasted their time and prostituted their talents in defiance of all that persuasion and punishment could do to prevent them; these too very frequently join in the groundless censure: however, those who have done their duty to themselves and to the University, whether they have been fortunate or not, know well that such censure is entirely without any foundation. On the other hand, they will at once agree in the assertion that the system pursued at Cambridge is quite unique, it may be termed indigenous; it has grown up there: if all the tutors, &c. were to leave the University, no other set of men could take it and go on with the system *now* followed there; this is a point, perhaps, worth consideration. All who have regularly gone through their *statu pupillari* and have acted up to college rules, are perfectly aware that the mass of matter passed over in the three years is immense, all of which the student ought to know well in order to pass a good final examination. Such, however, is the preparation made, and so ready are some of the aspiring candidates at that examination, that they sometimes actually write off answers to all the twenty or twenty-four questions within the limited two hours and call for more.

The intense application requisite to enable a man to gain high honours can only be known to those who have gone

through the ordeal. But instead of censuring the University for any sort of laxity in point of discipline, I am not sure that the rulers are not amenable to the penalties of Martin's act, for permitting the desperate preparations that are made for a run in their mental steeple chase. At all events, many a noble spirit has cracked in the tremendous efforts; others have reached the mark, but at the cost of their existence, or the entire wreck of their constitutions. Lovers of science and admirers of the most gifted of mankind must join in the wish that means could be devised, either by lengthening the time of preparation or simplifying the final examination, to prevent such melancholy results. As matters stand, a man must have the strength of a lion, coupled with the highest mental powers, to get high honours without committing suicide in the attempt. The subject deserves the attention of the University, and very probably does obtain their solicitude.

Having stated the extraordinary facility which some of the candidates for honours have in applying their knowledge, it may be asked how is that mental dexterity acquired? by what process has that surprising vigour of the mind been brought to such perfection? I answer, by a long course of proper drilling, chiefly by private tutors or friends, but very often in accordance with directions given by the tutors at the lectures; by continually solving problems given at the lectures by the tutors and others, for they are one and all anxious to assist a reading man : there is credit attached to answering questions quickly and neatly; this creates a stimulus. Solving questions at college examinations leads to distinction and college emoluments, so that the studious and talented undergraduate is in a continual round of problem-solving. To keep his mind steadily at it he has college friends and favours to entice and encourage him, and future honours of the most exciting kind to attract him. In my opinion it is this continuance of solving problems, this general course of not only acquiring principles but applying them, that at last makes the senior wrangler, who perhaps at the time is one of the most expert mathematicians in existence.

In our seminaries most noted for teaching mathematics I believe a somewhat similar course is followed. The mathematician in embryo is enticed by prizes and made expert by constant exercise in applying the principles that he has learned. Thus in all our institutions most celebrated for *making* mathematicians, not only is the practice of applying general principles to the solution of deductions, problems, &c. steadily followed, but inducements strong in proportion to the objects aimed at are held out to encourage the timid and impel the

ambitious. It is not intended to assert that mathematicians cannot be made without such inducements; some of our most celebrated ones have been their own tutor; but generally speaking, the above is the mode of *making* mathematicians, and it seems to be in conformity with the maxim, " In ediscendis scientiis exempla plus prosunt quam præcepta."

At our colleges and institutions most of our statesmen, lawyers, military men, &c., lay the foundation for their future eminence. It often does happen that even the senior wrangler gains no subsequent distinction; and we have seen the winner of the wooden spoon afterwards brilliantly work his way up to the bench, and then prove himself to be an ornament, even amongst the most distinguished judges. Let not, therefore, the unfortunate despair; but, generally, marked success at college or public institutions is a prelude to future eminence, and a very important step towards it.

But besides the classes just named who are sufficiently affluent or fortunate to be placed within the reach of all the advantages of tutors, &c. enumerated above, men who belong to the upper parts of society with respect to pecuniary matters, and who aim at professional distinction, there is another very numerous and important part of Englishmen who must not be overlooked, I mean the middle and lower orders. It will not be disputed that in a national point of view it is highly important that scientific knowledge should be spread as widely as possible among those classes; it will also, perhaps, be conceded that the foundation which they can lay at school is commonly of a very shallow description; all must therefore depend upon their individual application and talents subsequently exerted. I am fully aware that scientific books adapted for this class of students have been multiplied a hundred fold within the last thirty years. The nation owes the Society for the Diffusion of Useful Knowledge, and others of the kind, a deep debt of gratitude for originating the publication of such popularly useful works, and for exciting others to adopt the same laudable course. Every industrious man now, if he have talent, can improve it at a comparatively small expense. Mathematical treatises admirably adapted for the non-academic student are quite within his reach. But notwithstanding the facilities thus afforded to this kind of students, has all been done for him that might have been effected? I think not. Unless my view of the system pursued at Cambridge, &c. be altogether erroneous, mathematicians are there *made*, not by merely *reading* and *studying* mathematical works, but by applying their readings, and by the encouragements held out at examinations to the most successful competitors. The aca-

demic student feels that if he reads so that he understands his subjects, that he shall not only have an opportunity at his examination of displaying his acquirements, but that he will be rewarded in proportion to his success.

The non-academic student by dint of persevering study may make himself acquainted with all the mathematical sciences; but having accomplished that object, what opportunity has he of displaying his knowledge? The academic student has books selected for him; the non-academic student may possibly get books equally well adapted for him, but here anything like a parallel between their courses ceases. What inducement is held out to encourage the self-taught student? Very little indeed. The praiseworthy Society already named has been the means of putting ample stores of mathematical information within the self-taught student's reach; but it is to be regretted that that Society, or some other of like utility has not gone a little further and given him some encouragement to make use of them.

Some years ago there were the Lady's and Gentleman's Diaries, the Mathematical Companion, Whiting's Receptacle and Delights, the Leeds Correspondent, the Student, the Inquirer, Leybourne's Mathematical Repository, the Northumbrian Mirror, Gill's Mathematical Repository, and probably other publications of the same description which I have not: all these periodicals contained a number of mathematical questions, from simple elementary deductions up to the most abstruse problems. Prizes were proposed as inducements. Students of all grades of attainment had not only an opportunity of making known their acquirements in a manner most gratifying to themselves, but the prizes given in these highly useful publications had, in some measure, the same encouraging effect upon non-academic students as college prizes, &c. have upon students of a higher order. Unfortunately for the self-teaching student, the whole of the above scientific publications have ceased except the Lady's Diary. That esteemed periodical is luckily continued, and, under the guidance of the most distinguished talent, pursues its course of national usefulness. That little work has been published annually for upwards of 140 years, and it may fairly be asserted that that unpretending publication has been instrumental in making more mathematicians than any other English work that can be named. Almost all of our most celebrated mathematicians, during the period mentioned, have at some time contributed to its pages. There is scarcely any book of problems in the language that does not contain many of the gems which in the first place adorned the Diary, but the authors of the pro-

blem-books very commonly omit to name the fact: Mr. Davies, in page 21, vol. vi. of the Mathematical Repository, has made some very just remarks on this practice.

It is rather strange that this little periodical should have been allowed to run such a long career of important utility without obtaining some mark of national approbation ; but so far from encouragement did the state give it, that until very recently our fiscal directors had the gothic shabbiness to impose a heavy tax on it. Perhaps the whole history of literature does not contain so striking an instance of vandalism with respect to taxation.

Many of the publications mentioned were published by individuals distinguished by their acquirements and animated by a desire to be serviceable in promoting the spread of scientific knowledge, and in giving the self-taught student an opportunity of making his attainments known. These highly useful works were undertaken as a matter of pleasure, and not as a source of profit. I believe the printing of such publications is expensive. Some of the conductors were not men of fortune ; prudence therefore, at last, in some cases, was compelled to assume the place of inclination ; in others the veteran conductors had run their course. These are some of the reasons which account for those periodicals, so esteemed by the self-educated, and generally so much valued by all friends of mathematical and scientific knowledge, having come to an end.

The corporate bodies, which have done so much in diffusing scientific knowledge, have not yet taken any step to supply the non-academic student with a publication embodying the excellences of the several books referred to above. The societies adverted to have ample means at their command for supplying this desideratum. The outlay at first is frequently more than a man of confined means can conveniently advance. This to the societies named is an object of not much consideration ; however, there need be no apprehension of loss were the publication made attractive in every form, that is, by giving prizes for distinguished merit ; by giving a number of questions sufficiently varied for the profound mathematician to display his acquirements, and for students of every grade to show the extent of their attainments. The publication should also afford room for the philosophic student to exhibit his lucubrations, for the literary man to make known his learning and taste, and for the young aspirant in literature to put proofs of his ability in *print*. The societies already mentioned profess to have objects in view quite paramount to pecuniary ones, namely the diffusion of knowledge amongst the people; it is

in that character, which I admit they have well sustained, that these remarks are addressed to them.

The Diary at present is the only publication that I know in England that aims at the objects above enumerated; no praise of mine could tend to enhance the general esteem for that truly useful little publication; but the whole work, even in its enlarged state, which is devoted to poetry, philosophic queries, mathematical questions and papers on mathematical subjects, consists of only eighty-one small pages, published once a year. To this small space there are nearly one hundred literary contributors, and at least fifty scientific correspondents. Although the prescribed space is divided amongst the contributors with the utmost impartiality, it must be obvious that there is not room for a proper display, that many valuable communications must necessarily be thrown away for want of room, and that however anxious the highly talented editor may be to oblige the whole of his host of correspondents, many of them every year must be disappointed. With such narrow limits of accommodation cold water must be thrown on the anxious aspirings of many an ardent student. So high in repute does the Diary deservedly stand, that it is sure to have a crowd of correspondents; but is it not to be regretted that there is not more room, that so much valuable matter should be thrown away, that the arena—the only arena—for English non-academic students to display their abilities and acquirements should be so confined?

My object in writing this paper would so far miscarry if I did not mention everything which, in my opinion, is likely to prove serviceable to the non-academic mathematical student. In the absence of publications affording him room and exercise for his abilities he will find mathematical questions every week in the York Courant. Amongst the contributors he will see the venerable names of Butterworth and Whateley, very accomplished mathematicians, and others of note who have enriched our scientific literature by their labours during many years.

There is another publication recently commenced which richly deserves the student's attention, namely, The Mathematician, by Messrs. Davies, Rutherford and Fenwick, of the Royal Military Academy, Woolwich. This celebrated academy is truly a national one; the establishment has justly entitled itself to the nation's esteem, besides qualifying some of the most scientific military officers in Europe. Its professors and tutors have always been noted, not more for their splendid attainments than for their popular usefulness; they have always rendered their talents eminently beneficial to the non-academic

student.   The works of Simpson, Hutton, Barlow, Gregory, &c. are quite familiar to the self-taught student; their names to him are like household deities.   Non-academic students have long regarded the works that have emanated from the Royal Military Academy as their text-books; so that the academy has been a national one in every wide application of the term. The professors still maintain with becoming credit the celebrity of the establishment: the three gentlemen who have undertaken the editorship of The Mathematician are well qualified to maintain and increase it.   Two numbers of the periodical have been published; it is exclusively mathematical; it is intended to supply the place of Leybourne's Mathematical Repository; the essays by the editors and others of kindred talents are of the most masterly description.   The Mathematician is adapted for the higher order of students, but every lover of mathematics will find it a gem of the first water; it deserves public support, and there can be no doubt of its obtaining extensive circulation.

With regard to mathematical questions, the editors, in their prospectus, having adverted to the Mathematical Repository, say, "It is our intention to curtail in some degree the department of mathematical questions, for though we are fully impressed with a sense of the importance of this feature of the work, universal experience shows the difficulty of forming a sufficient number of new and good questions where a fixed number must be made up by a given time; and the insertion of such as lead to mere petty details of calculation and deduction, suited only for the student's private exercise, tends not only to lead him into frivolous researches, but to create a false taste in science.   We shall hence insert only such as involve some new principle, or require for their solution some new modes of investigation, such as either lead to results remarkable for their unexpected simplicity, elegance and symmetry, or tend to the extension of an old or to the commencement of a new and valuable course of inquiry.   These and these only will find a place in the present work; and as we do not confine ourselves to any exact number of questions or our correspondents to the time when they transmit their answers (leaving this to their own convenience), we hope to render this department free from the reproach so often applied to works of this class, that of '*creating a race of mere problem-solvers.*'"

It is obvious from this quotation that the editors intend their publication for the higher order of mathematicians.   Their prescribed kind of questions can only be expected from experienced men; their rule is so stringent that it forms a prohibition to all juniors.   Mathematicians, in their very neat pub-

lication, may certainly find a rich treat, but it is intended for mathematicians already *made*; how they are to be made, or make themselves, are other questions.

It was with a feeling very like regret that I first read the concluding part of the preceding citation, that is, " we hope to render this department free from the *reproach* so often applied to works of this class, '*that of creating a race of problem-solvers.*' " I wonder who the cynics were that uttered this reproach; depend upon it they were no problem-solvers. If I knew them I would venture to pay them a little attention in passing, even at the risk of being bitten for my politeness; but be the snarlers who they may, it would be difficult to find these gentlemen better qualified, and I should have supposed more fully prepared, to defend the race of problem-solvers from the aspersion, than the three talented editors themselves: this being my impression, I was not a little surprised and sorry to learn that they had shaped their course at all agreeably to the tendency of that reproach; if my influence with them were half as great as is my respect for their accomplishments, I would induce them to modify that part of their plan; by doing so, in my opinion, they would very considerably extend the usefulness of their praiseworthy undertaking.

The race of problem-solvers, however, in answer to such reproaches, might exultingly refer to the results of merely solving problems. The theory of the tides, much that is valuable in physical astronomy, various branches of dynamics, &c., owe their neatness and perfection to the solution of some problem once proposed as a challenge or a prize. You cannot read a treatise on the theory of numbers or on any subject connected with the higher branches of the mathematics without finding ample proofs of the fact. All mathematicians are "*problem-solvers*;" there is scarcely an English mathematician of any note who has not joined or does not at present belong to the *race* of problem-solvers. As a mental discipline, the *theory* of the mathematical sciences would deserve esteem; but it is not easy to see how they can be actually applied to useful purposes, except through the medium of problems. I suppose that all problem-solvers must have a commencement; they can hardly begin at the top, but must work up from questions of mere calculation and petty detail.

Prof. De Morgan, in a very interesting article in the British Almanac of this year, says, " The higher classes of mathematicians at the end of the seventeenth century became excellent computers, and this was particularly the case in England, of which Wallis, Halley, Newton, the Gregories, and De Moivre, were splendid examples. . . . . . . During the last century,

however, a schism has taken place among mathematicians of our country; those not educated at the universities have preserved the art of computation, while those who come from the universities have well nigh abandoned it entirely. Let any one compare the questions proposed in the Lady's or Gentleman's Diary, the classical works of the non-academic student in England with those given to the candidates for honours at Cambridge, and he will see that the problems which are to be answered numerically are at least three times as large a per-centage in the former as in the latter. We regret this prevailing fashion of the University of Cambridge; we think that a little systematic attention to the acquisition of arithmetic expertness would very much increase the amount of power acquired by the students. We think also that the demand for such qualifications would act most advantageously upon the preparatory education."

I have made this rather long quotation instead of simply referring to the article, in the hope that it may meet the eye and arrest the attention of writers who can speak reproachfully of questions of calculation and detail. The regret which Mr. De Morgan professes to feel for the prevailing fashion at Cambridge may, I think, be extended more generally. Within the last few years there has been, or I fancy there has been, a tendency to adopt the fashion referred to in works intended for non-academic students. I wish by no means to depreciate mere theoretical problems; perhaps there ought to be a fair proportion of that description; but when we consider that the greater number of our really working mathematicians belong to the non-academic order, that they commonly commence their career in the publications alluded to and complete their qualifications in some measure according to the models there given, it is, I think, of very high importance that questions of calculation be duly encouraged, for unless a mathematician can carry out his theory into numerical calculation, he may be a very learned and ingenious man, but he can hardly be said to be a useful one. It is possible by dint of drilling or application for a man to write out correctly, in symbolical language, solutions to the most abstruse questions, and yet be not a little puzzled to put the result in figures so that common understandings may know what it means. I sincerely admire the talents and attainments of men who can thus write in theory; but I should not esteem them the less, if, at the same time, they were expert in adding up their tailor's bill.

The mere theorist may revel *in nubibus,* but he must come down to terrestrial matters of numerical calculations before he can be of much real utility to his fellow men.

There is another point to which I wish to call the attention of gentlemen who have the guidance, or who feel an interest in the progress of the non-academic student; I mean an apparently increasing partiality for mathematical essays or memoirs. In dealing with this subject I wish to speak very carefully. I very readily grant that any one who reads a memoir in a foreign language, sees its utility to the English student, and knows that it is not to be found in English books, becomes a benefactor to science if he puts it in an English dress, *states its origin*, and gives it circulation. Nor must those ingenious essays, intended to illustrate intricate subjects, or to simplify difficult ones, when executed by competent persons, be undervalued; each and all of these labourers in the vineyard of science will, it is hoped, go on and prosper. But there are essay-writers of another kind; I mean those who crib scraps from French or other foreign works, cook them up in English fashion and pass them for their own. They steal the offspring of others' brain, darken its features by some disguising explanation, and then, gipsy-like, wish it to be taken for their own. To pass off a cribbed article as an original one, a clap-trap notation must be particularly accommodating; that hocus pocus disguise must be of great assistance; of course it is elegant because it mystifies, and it is convenient because a small number only know what it conceals. Mr. Davies's opinion upon such articles may be seen in different parts of the sixth volume of the Mathematical Repository; he there promises a dissertation on an inquiry into the principles which should regulate our judgement in deciding upon mathematical plagiarism, with examples from a certain class of mathematical writers in this country. I have not yet seen the dissertation, but it is likely to prove a very interesting *vade mecum* to the whole race of problem-solvers.

But admitting that mathematical essays, illustrations, memoirs, or whatever name they are called, are really authentic and genuine, perhaps it may be doubted whether they are so generally useful to the reader, particularly to the self-taught student, as the old-fashioned mathematical problem, question, or, if you please, mere deduction for exercise. An article illustrating a difficult point or putting a subject in a new light may be valuable; it may evince originality and talent, and if it formed part of a treatise on the subject would add to its value; but it is very possible that the subject is more interesting to the writer than it is to readers generally; it may be on some recondite topic that few understand and fewer still care about; in such cases, if the articles have any tendency at all, it is more to the glorification of the writers than to general use, whereas a problem is something like a challenge to the

ingenuity of all. The mathematical essay may obtain the student's perusal and possibly add to his knowledge; but I do not think it is so likely to arrest his attention and set his abilities to work as a set of problems adapted to his acquirements or going a little beyond them would be.

Had the preceding observations been steadily kept to the point instead of being desultory in the widest sense of the word, they might have ended here; but now I must beg to trouble the reader with a brief recapitulation, to set before him more distinctly the object of this article.

The importance of the mathematical sciences has been taken for granted, and it has been endeavoured to make known the facts, that the mode of cultivating them at Cambridge and at other seminaries of lesser note is by continual exercise in applying principles to the solutions of problems, &c.; that the student is not only exercised in this way, but that he is also enticed and encouraged by prizes and honorary distinctions, well adapted to make him exert his abilities to the utmost. That this is the plan pursued at the fountain-head of mathematical education is well known, and perhaps it is the best scheme to effect its purpose which the human intellect could devise. It has been assumed, that it is obvious that only a small part of the community can participate in such advantages, and that it is equally obvious that, taken in any point of view, a diffusion of the mathematical sciences among the classes further down is of high concern; and here I would wish to advert briefly to the moral effect. It has been said, and in my opinion justly said, that "he who knows the first proposition of Euclid is, in so far, better than he who does not" (Edinburgh Review, No. 154, p. 376). This assumption must not be misunderstood for the purpose of cavilling; no undue preference is claimed for the mathematical sciences with regard to other knowledge, but as an exercise to keep the mind in a right direction none can fairly be placed higher, and consequently he who knows the first proposition of Euclid has acquired food for the mind which, to that extent, stops up the inlets to error, and he is more unlikely to take a wrong course than he who has a mind entirely vacant, and is therefore so far better. Even then, under a moral consideration, as a mere mental discipline, the utility of scientific knowledge to the most numerous part of our countrymen is worth attending to. But seeing that these classes furnish nearly all the working part of our scientific men to whom a knowledge of the mathematics is almost necessary, certainly highly desirable, I think the advantages that must arise from the encouragement of those sciences amongst these classes must be manifest.

The mathematics were never more sedulously cultivated at

the universities than they are at present. I have laboured to show the manner by which mathematicians are there made. No scheme can be devised to put the non-academic student on a level with the more fortunate academic one, but it has been my desire to contend, and my wish to prove, that all has not been done for the self-taught student that might be, and, as I think, ought to be done. Books well written on all subjects are within his reach, but the inducements to make him read them are, according to my view, exceedingly circumscribed. It has been attempted to show, that to the non-academic student, publications, not too costly, containing a number of proper questions, and holding out prizes for stipulated solutions, supply, to a certain extent, the place of college examinations, prizes, &c.; each excites a spirit to excel and becomes an incentive to mental exertion. At present the Lady's Diary is the only work of the kind published in Great Britain; consequently, as I think, non-academic and self-taught students are not properly encouraged; the public does little or nothing for them; the societies so often referred to might, it is thought, very easily supply this national defect, if they could be induced to do so. I fear my immethodical remarks will not effect much, but I will indulge the hope that they may call the attention of some one to the subject who has the power as well as the wish to render some service to the self-taught and non-academic student.

XIII. *Note on the Heat produced during Metallic Substitutions.* By THOMAS ANDREWS, *M.D., M.R.I.A.*

THE following general principle, which is a probable consequence of my former investigations*, I have succeeded in establishing by numerous experiments. It is a very simple law, and refers to chemical reactions, the interpretation of which is equally simple. The principle may be thus stated:—

"When one and the same metal displaces another from any of its neutral combinations, the heat developed is always the same; but a change in either of the metals produces a different development of heat."

The most important source of error to which the experiments in proof of this principle are liable, arises from the formation of voltaic circles by the precipitated metal with the excess of the other metal. The slight voltaic action thus induced produces a sensible amount of heat; but by conducting the experiments under suitable conditions, the error from this cause may be almost entirely avoided.

Belfast, June 29, 1844.

[* See Phil. Mag. S. 3. vol. xix. p. 183.—EDIT.]

XIV. *Strictures on* Professor Dove's *Essay " On the Law of Storms," introduced by Remarks on* Mr. Redfield's *recent communication on that subject. By* ROBERT HARE, *M.D., Professor of Chemistry in the University of Pennsylvania.*

To the Editors of the Philosophical Magazine and Journal.
GENTLEMEN,

I OBSERVE that in the Supplementary Number of your Journal for January 1844 [S. 3. vol. xxiii. p. 481], Mr. W. C. Redfield makes great complaints of my apparent want of candour in quoting his opinions and neglecting his replies. According to my opinion the errors committed by Mr. Redfield may be advantageously distinguished into two classes : in one, those urged in support of his theory of storms should be placed; in the other, such as have arisen from his misapprehension of my motives and my arguments. Excepting in a few cases, I do not propose to occupy the pages of your valuable Journal with any notice of the latter class. In publishing my objections to the whirlwind theory, I did not flatter myself with any expectation that they would change Mr. Redfield's opinions. I anticipated that one who could construe the phænomena of nature in a way so repugnant to my idea of science and reason, would not be likely to accord with me in the validity of any objections which I might urge. Hence I announced the intention of not addressing my arguments to him, and it is my opinion that were I to do so it would only multiply our differences.

The more I contemplate the whirlwind theory, upon the assumed correctness of which Mr. Redfield and his disciples were circulating instructions to mariners which I deemed fallacious, the more was I convinced of the utter incompetency of any evidence which could be adduced to establish an hypothesis involving so much that was inconsistent with all that I knew of the laws of motion, and the meteorological efficacy of heat and electricity. This opinion was strengthened when I found that the observations by which his theory was represented as established, beyond the reach of my objections, were replete with contradiction and inconsistency.

Aware that while entertaining these impressions it might be difficult to do justice to what I considered as the truth, without using language which might seem to be disparaging, in sending my additional objections to Professor Silliman for publication, I requested him to point out any of my language which he might deem unduly unpleasant; accordingly, all that to which my judicious friend objected was modified or left out, though very little change was advised. I have mentioned these facts in order to show that it has not been from any want

of indisposition to give Mr. Redfield unnecessary pain, that I have so expressed myself as to cause his displeasure.

Agreeably to the notice subjoined to the essay entitled ' Additional Objections,' published in the Number of your Journal for August last, I send you a copy of my strictures on Dove's Law of Storms.   By the next opportunity I will send another communication, in which I shall advance some further proofs, that if such whirlwind storms as have been imagined by Redfield, Dove and others, could be created by the causes which they have suggested, they could not endure, after the cessation of the generating forces, longer than might be requisite to restore the equilibrium deranged by the centrifugal force consequent to the gyration.

<div style="text-align:center">I am, Gentlemen, respectfully,
Your obedient Servant,</div>

Philadelphia, February 27, 1844.   ROBERT HARE.

104. I have not been enabled to discover that Professor Dove attempts to assign any cause for violent winds.   Assuming that a wind, sufficiently violent, is blowing from south to north, he ingeniously makes a new application of the old doctrine of Halley, by which the westerly motion of the trade winds is ascribed to the diversity of the velocity of the earth's surface, at different distances from the equator, operating upon a wind blowing from one parallel of latitude to another.   I am however unable to understand how any difference of momentum, thus arising, can act throughout all parts of a circle upon an elastic fluid, so as to sustain the equability of motion requisite to enduring gyration. It seems to me that the influence of the terrestrial motion can operate harmoniously neither upon each quadrant, nor each zone of a circle.   The effect upon the south limb cannot, I think, cooperate with that upon the northern one.

105. Moreover, as the velocities of the aëriform particles in a whirlwind must be greater as they are further from their axis, I do not see how a uniform force operating upon particles requiring such various velocities, can produce movements which can harmonize in causing a non-conflicting rotation of the whole mass.

106. How can this process avail to produce a revolution in the same direction in all the storms of this quarter of the globe, as alleged by Mr. Redfield and sanctioned by the author, when, agreeably to the most ample and satisfactory evidence adduced by Professor Loomis, as well as general experience, some of the most violent storms of this continent travel from the north-west towards the south-east? In such

cases, on account of its blowing obliquely towards or from the
equator, the wind would change its position relatively thereto,
only with a portion of the speed which is assumed in the cal-
culation of Professor Dove, and when the change of position
should be in an opposite direction from that which he supposes,
would it not cause the storm to whirl in the opposite way?

107. Professor Dove in the second paragraph, p. 211, em-
ploys the following language:—"As the West India hurricanes
originate at the inner boundary of the trade winds, where, at
the so-called region of calms, the air ascends and flows over
the trade in an opposite direction, it is probable that portions
of this upper current, penetrating through the lower one, can
give the first occasion of those storms. The high mountains
of several of the islands, by offering a mechanical impediment,
may be one cause of this effect, as the air flows with redoubled
violence between two mountains."

108. Professor Dove here alleges that the upper current
may penetrate the lower, but does not say why it should do so.
Wherefore, it may be demanded, should the upper current
penetrate through the lower current, and supposing it to do
so, why should it be productive of a hurricane?

109. The Professor goes on to say, "It is evident, that if
the above deduction of these phænomena be the true one, a
similar whirlwind must be produced wherever, owing to any
other mechanical cause, a current flowing towards a high
northern latitude is more southerly on its eastern side than on
its western."

110. It seems as if Professor Dove, no less than Mr. Red-
field, falls into the error of making the *cause of gyration* the
only object of inquiry. It is, according to them, sufficient to
show that the rotation of the earth, or the reaction of a moun-
tain, may give a curvilinear direction to the wind. To ac-
count for the wind itself is not in the least necessary!

111. Can anything be more inconceivable, than that a cur-
rent of air, not previously moving with the force of a hurri-
cane, should, by influence of the earth's motion, or a conflict
with one or more mountains, be excited into a tempestuous
fury? Whence comes the alleged peculiar violence of the
whirling portion of the atmosphere noticed in such storms?
Evidently *deflection* could not cause any *augmentation* of force.
The velocity of the whirl would be less instead of greater than
that of the generating gale, since the centrifugal force conse-
quent to rotary motion would be productive of a collision with
the surrounding atmosphere, tending to dissipate the momen-
tum. This, as I have already observed, could receive no re-
inforcement, while the mass actuated by it would increase with

the square of the distance from the axis. (See Additional Objections to Redfield's Theory, par. 82.)

112. Professor Dove has not considered the incompetency of a *local* cause of deflection to beget permanency of rotation in a *travelling* storm; nor the impossibility of the endurance of a momentum sufficient to cause the violence of hurricanes without continuous exciting forces.

113. In a passage which I shall in the next place quote, the idea is advanced that the axis of a whirlwind may incline forward so as to cause the higher portion to precede the lower, and to make the lower stratum of the air forming the whirl exchange places with the upper stratum. This view of the phænomena I shall endeavour to prove erroneous.

114. In Taylor's Scientific Memoirs, already cited, vol. iii. p. 215, paragraph 3, Professor Dove has thus expressed himself:—" In considering the progressive advance of the whirlwind, we have not hitherto taken into account the resistance opposed to the motion of the air by the surface of the earth. This resistance, as Redfield justly remarks, causes the rotating cylinder to incline forward in the direction of its advance, so that at any station the whirlwind begins in the higher regions of the atmosphere before it is felt on the surface of the earth, where therefore the sinking of the barometer indicates its near approach. The inclined position of the axis causes a continual intermixture of the lower and warmer strata of the air with the upper and colder ones, thereby occasioning heavy falls of rain and proportionably violent electric explosions."

115. In order to appreciate the fallacy of the ideas above presented, it should be recollected that the "*rotating cylinder*" of air, which is represented as *inclining forward*, can receive this name only because the portion of the atmosphere of which it is imagined to be formed, is conceived to revolve within that cylindrical space which must of necessity be occupied by a whirlwind. To justify this appellation, the gyrating particles must all move in concentric circles about a common axis, and between planes parallel to each other and at right angles to that axis. Any other rotative position of the parts must be inconsistent with enduring rotation, since it would bring different parts of the mass in collision with each other and with the air beyond the sphere of the gyration. It should be recollected also, that agreeably to observation, hurricanes have been estimated to extend from 100 to 600 miles in breadth. Let us assume the diameter of a whirlwind storm of this kind to be 360 miles. Of course the circumference being about three times as great would have three miles for every degree. It follows, that a vertical circle of the diameter of the storm, in a plane coinciding with the axis, would also have three miles

for every degree. The altitude of storm-winds is well known not to be above two miles, so that the diameter would be at least 180 times as great as the altitude of the axis. Can such a cylindrical mass of air be conceived to incline forward? One degree of inclination would lift the base in the rear three miles, and two degrees would lift it to the height of six miles, which is never attained by clouds. Besides, as the density of the air in regions so elevated is only one half of that upon the earth's surface, is it conceivable that a whirlwind could consist of materials so disproportioned in weight?

116. Can the suggested process of circulation proceed when, in order for the lower stratum to exchange places with the upper, it would have to move nearly half of the circumference of the storm, or more than five hundred miles?

117. It has been shown that the *rotating cylinder* of air which constitutes a hurricane, agreeably to the language employed, and the theory espoused by Prof. Dove, may consistently with the observed dimensions of storms, have a diameter two hundred times as great as its altitude. The base, of this flat cylindrical aëriform mass, must be in contact with the terrestrial surface, and of course in collision with its rugosities and inequalities, while all the rest of the rotating superficies, being contiguous to inert particles of the atmosphere, must incessantly share with them any received momentum. Is there any known cause of motion in nature which can impart to a fluid and elastic mass so formed, composed and situated, the various velocities necessary to that simultaneous rotation of the whole which the creation of a whirlwind requires? In answering this question, it should be recollected that the velocities must diminish from the zones of which the gyration is most rapid, towards the axis on one side, towards the circumference on the other.

118. Evidently no transient impulses can produce harmonious revolution throughout the mass, unless they act upon every particle so as to impart to each the peculiar velocity which its distance from the axis may require; and any enduring cause operating partially, could only affect the whole by a gradual process of participation which would cause it to be expanded beyond as well as within any "*rotating cylinder*" which might be created.

119. But admitting that in such a mass, under such circumstances, the gyratory violence of a hurricane could be induced, could this violence be sustained, after the cessation of the generating forces, merely by the rotatory momentum of an enormous aëriform disc, formed, proportioned, supported, and surrounded as the whirlwind above imagined must be, could any such exist?

120. One of the grounds taken by Prof. Dove, appears to me strikingly untenable. His language, vol. iii. p. 214, last paragraph, is as follows:—" The dead calm, suddenly interrupting the fiercest raging of the storm from opposite directions, which is shown in the register of observations at St. Thomas,—that dreadful pause which fills the heart of the bravest sailor with awe and fearful expectation,—receives a simple explanation on the rotatory theory, which requires that at the centre of the whirlwind the air should be in repose; but appears irreconcilable with the supposition of a centripetal inblowing, because two winds blowing towards each other from opposite directions must gradually neutralize each other, and thus their intensity must diminish more and more in approaching their place of meeting. This takes place on the great scale in the trade winds, and if the centripetal view of hurricanes were the just one, the same effect would necessarily be seen as the centre of the storm passed over the station of observation. But the phænomena shown by observation are widely different. At St. Thomas the violence of the tempest was constantly increasing up to $7^h\ 30^m$ A.M., when a dead calm succeeded, and at $8^h\ 10^m$ the hurricane recommenced as suddenly as it had intermitted. How can this be reconciled with the meeting of two winds?"

121. I have made the preceding quotation from Prof. Dove's essay, conceiving it to contain evidence which must be fatal to the hypothesis which it is intended to prop. It establishes that in hurricanes the wind is liable *suddenly* to subside from its extreme violence to a calm, and then as suddenly to recommence blowing with as great violence as ever in an opposite direction. I am very much mistaken if I have not in my Additional Objections to Redfield's Theory (79 to 84) demonstrated that, in *extensive* whirlwinds, the "*fiercest raging*" cannot be suddenly interrupted so as to leave a dead calm during the interval which takes place between two opposite winds; since in such storms, where they have a diameter not less than three hundred miles, for the same station to be exposed successively on opposite sides of the zone, where the wind is most violent, the storm must move at least one hundred miles, which would require from three to four hours.

122. Referring the reader to the essay above mentioned, I will urge, in reply to the query already quoted, that Prof. Dove's allegation that " *winds blowing* from opposite quarters *will neutralize each other*," arises from his forgetting that agreeably to the hypothesis which he is striving to confute, they are caused by a deficiency of pressure at the axis of the storm, producing an upward current for the supply of which they are

required. It could not but be admitted by the learned Professor, that when, under such circumstances, a fluid rushes from all quarters towards a focal area, the consequent motion must quicken as it approaches the ascending current. It must also be clear that when it moves from all parts of the circumference, the velocity must increase inversely as the square of the distance from the centre. Nevertheless, after the base of the ascending column is reached, evidently the horizontal afflux must be superseded by a vertical movement. Hence about the centre of the space around which the upward currents prevail there may be a calm.

123. It seems to be conceded that a tropical hurricane is a gigantic tornado. Of course it may be assumed that the features of these meteors are proportionable; and that the focal area of a hurricane will be as many times greater than that of a tornado, as the whole area of the former is greater than that of the latter. In fact, if the focal areas be respectively bases of ascending columns moving with equal velocities, the quantity of air requisite to supply the upward currents thus constituted, will be as the squares of the diameters of the columns severally.

124. The diameters of the focal areas of tornadoes as observed in this country, seem in no instance to have exceeded 500 feet. The focal area of the Providence tornado was estimated to be 300 feet in diameter*. To supply an upward columnar current of 10,000 feet diameter, would require 400 times as much wind as to supply an analogous current of a diameter of 500 feet. It follows that a hurricane equivalent to 400 of the largest tornadoes, would not require a focal area greater than two miles in breadth. To cross this at the rate of progression attributed to great storms, by Prof. Dove (thirty miles per hour), only four minutes would be necessary.

125. Let us suppose the focal area of a hurricane, forming the base of a vertical current, to be in diameter 10,000 feet, and that the space beyond the area be divided into zones by the circumferences of circles equidistant from and concentric with each other and with the focal area. The circumferences being equidistant, the quantities of air over these zones severally will evidently be as the squares of their mean diameters. Of course if the zone nearest the area, having a mean diameter of 10,500 feet, move inwards with the velocity of 100 miles per hour, the velocity at four times that distance, or 42,000 feet cannot be more than one-sixteenth as great, or little more than six miles per hour. Thus at only four miles from the centre, the centrifugal velocity would scarcely be adequate to a breeze.

* See letter from Z. Allen, Esq. in Silliman's Journal, vol. xxxviii. p. 76.

126. This calculation, founded on the idea of the confluence of the air equally from all points of the periphery, would seem too much to contract the theatre of great storms; but in point of fact, it probably never happens that there is a confluence of the wind from all quarters. In the storm of which the phænomena are so well recorded by Prof. Loomis, the wind blew principally from two opposite quadrants. But in either case the influence of the inward suction must diverge and diminish in force as the distance from the focal area increases, so that the greatest violence will be in the vicinity of its border, where the wind is most concentrated. For as soon as the confluent currents get within the border, they must be deflected upwards; and thus the central space must escape their influence; excepting the diminution of pressure consequent to the upward motion.

127. It is I hope thus rendered evident, that the facts adduced, in the quotation above made from Prof. Dove's essay, are quite consistent with the idea of winds rushing towards a focal area, while they are utterly irreconcilable with that in support of which they have been brought forward.

128. I would recommend Prof. Loomis's* observations to the candid attention of Prof. Dove, and would request him to show in what manner the earth's motion cooperated to produce it; or how the enormous length of the focal area, or area of minimum pressure, comparatively with its breadth, can be reconciled with the idea of its having formed the centre of an extensive whirlwind. There is another fact which would seem to be an insurmountable obstacle to the rotation of a storm travelling from the valley of the Mississippi to the Atlantic coast. I allude to the interposition of the Alleghany mountains. Prof. Dove's imaginary aërial cylinder would be cut nearly in twain when bestriding that range. Obviously more than one half of the air in such a cylindrical mass would be below the average level of the summits of those mountains. Under such circumstances could it be conceived to rotate about a vertical axis?

129. I am aware that various writers have referred to the little *transient* whirls which are occasionally seen to take place in a windy time, carrying up dust, leaves, and other light bodies, as a support for the idea of whirlwind storms; and Mr. Redfield has alleged, "that no valid reason can be given why larger masses of air may not acquire and develope similar rotative movements."

130. It appears to me that there are several valid reasons for not adopting the view of the subject which he has taken.

* See American Philosophical Transactions for account of the storm of December 1836.

The momentum by which any body is kept in motion, is as its weight multiplied by its velocity, while the expenditure of momentum is *cæteris paribus* as its surface.   On this account, a globe of which the content in proportion to its superficies is pre-eminently great, will, in a resisting medium like the air, retain a rotatory motion longer than an equal weight, of the matter forming it, in any other shape.   The flat cylinder, in diameter about 200 times its thickness, of which the existence would be necessary to an extensive whirlwind, is a form of which the surface would be very great in proportion to the quantity of matter which it contains.   No observer ever noticed any whirl produced as above described, to have a diameter many times greater than its height, or to endure many minutes. Such pigmy whirls appear to be the consequence of eddies resulting from the conflict with each other, or with various impediments, of puffs or flaws of wind.   No doubt in this way a deficit of local density is easily caused in a fluid so elastic as the air, and consequently by gravity as well as its elastic reaction, a centripetal motion is induced in the surrounding aërial particles.   From the confluence and conflict of the air thus put into motion, a whirl may arise.   The manner in which light bodies are gathered towards the axis of these whirls, shows that they are accompanied by a centripetal tendency. It is only when the wind blows briskly that such whirls are ever seen to take place ; but tornadoes, agreeably to universal observation, occur when there is little or no wind externally (see par. 93).

131. According to the evidence adduced by the advocates of the whirlwind theory, there is in this respect perfect similarity in the phænomena of tornadoes and hurricanes.   Beyond the sphere of the alleged gyration, there is but little if any atmospheric commotion, and certainly none competent to be the cause of a great whirlwind.   It follows that pigmy whirlwinds and hurricanes can have no analogy.   The former are never produced without a proportionable external activity in the wind, while comparative external quiescence seems to accompany the latter.

132. I will conclude by applying to Prof. Dove the stricture which I applied on a former occasion to Espy and to Redfield.   He has, I think, committed a great oversight in neglecting to take into consideration the agency of electricity in the generation of storms.

[The memoir of Prof. Dove, to which this communication of Dr. Hare relates, will be found in Taylor's Scientific Memoirs, vol. iii. p. 197.   In Dr. Hare's quotation from it, *suprà*, p. 96, lines 9 and 10, some words are omitted: instead of " at the so-called region of calms," it should have been, " at the limit of the so-called region of calms :" and in the last line of the quotation, for " violence " read " velocity."—Edit.]

XV. *Further considerations on the Existence of a Fourth Imponderable. By* JOHN WILLIAM DRAPER, *M.D., Professor of Chemistry in the University of New York*[*].

IN the Philosophical Magazine for December 1842, I brought forward several facts which had caused me to form the opinion that the chemical rays of the older optical writers constitute in reality a new imponderable substance, which should be placed in the same rank with light, heat and electricity. To the views then given I propose, in this communication, to return again, and furnish further proof of their correctness. An extended examination, which has occupied me several years, has served to deepen my conviction of the truth of this doctrine.

Great changes in the fundamental theories of science ought not to be lightly admitted. It was only after many years of discussion and multitudes of experiments that the doctrine of the unity of air was destroyed, and the theory of the intrinsic differences of gaseous bodies received. This was unquestionably the most important event that ever happened to chemistry.

The imponderable principles are the true LIVING FORCES of chemistry. The circumstance that they do not exhibit the property of weight is only an incidental affair, and ought never to have been regarded as their leading characteristic; they are the regulating forces by which ponderable matter is arranged and grouped. If, then, so great a change occurred in chemistry on more exact views being obtained of its pneumatic department, what may not be expected from the discussions which are arising on the nature of its great controlling forces?

There is another point of view from which these investigations assume a deep interest. I have shown (Phil. Mag., Sept. 1843) that by resorting to prismatic analysis in physiological researches, very remarkable truths appear. The function of digestion, which is carried on during sunshine by the leaves of plants, is under the control of the yellow ray. It is this which causes the decomposition of carbonic acid, furnishes solid food, and gives the green colour. In animals similar results are produced by the agency of a nervous system; and not only so, but all the various operations connected with life are conducted in the same way. There is one class of nerves which gives action to the respiratory apparatus, and another which controls digestion. There is one class which presides over motion, another which is the recipient of sensation, a third which originates all the processes of thought and intellectuality. In the vegetable world the same idea is preserved, de-

[*] Communicated by the Author.

veloped perhaps in a less elaborate way, but under the guidance of a principle equally æthereal and refined. *The beams of the sun are the true* NERVOUS PRINCIPLE *of plants*. To the yellow ray is assigned their nutritive processes, to the blue their movements. We can therefore easily understand how it is that botanists who have sought in the interior of plants for indications of a nervous agent have never found them. That agent is external.

By the experiments that have been made for determining the nature of the chemical radiations, the question has been brought down to very narrow limits. There is no author who regards them as connected in any way with radiant heat, nor any, except the wildest speculator, who traces them to electricity. The difficulty is to offer clear and undoubted proof that they are distinct from light. Herschel has directly admitted this distinction and brought forward several experiments (Phil. Trans. 1840, p. 38, &c.) in support of this view. I have given some evidence of the kind, both recently and also several years ago (1837). All these experiments depend on a comparison of tithonographic stains produced by solar spectra that have undergone the action of absorptive media, and the effect of those spectra on the organ of vision; a comparison, in short, of different visible spectra and their tithonographic impressions.

From this comparison we endeavour to prove that invisible rays may be isolated in any part of the spectrum, and, if invisible, we argue that they are not light.

It cannot be concealed, however, that there is a certain degree of imperfection in this species of evidence: an accurate conclusion as to the presence or absence, or quantity of light, is by no means under these circumstances an easy affair. In these distorted spectra, as in the natural one, the terminations shade off gradually, and it is difficult to say where the light in reality ends. On these terminations also, where the light is so dilute and feeble, tithonographic action, although faint, may, by prolonged exposure, become not only perceptible but even prominent. In tithonographic action time enters as an element, in the act of vision it does not. A feeble gleam does not become more bright by constantly looking at it, but a sensitive surface exposed to such a gleam is more and more affected as the time is increased.

Considerations like these demonstrate the necessity of investigating the question in other ways, and more especially since M. Becquerel, one of the ablest writers on these matters, has undertaken to support a doctrine which denies the existence of the chemical rays and imputes the whole action to light (Taylor's Scientific Memoirs, vol. iii. part 12).

This doctrine, however, will not, I am persuaded, stand the test of criticism: there are facts, and those very imposing ones, which make it utterly untenable. It is my object in this paper to set forth that evidence and offer further and clearer proof of the physical independence of the tithonic rays and light, and indirectly establish the existence of a new imponderable.

The true issue of the question, as has been said, rests in proving a clear distinction between light and the tithonic rays; the other imponderables may be left out of the argument. The mechanical properties of the two agents are so closely alike, reflexion, refraction, polarization, interference, &c. taking place under the same laws for both, that the discussion necessarily becomes one of quantity and measure. Will a given ray of light, disturbed by the action of absorptive media, change its luminous and chemical relations *pari passu*? or can we alter the one and leave the other untouched? or, changing both by any process of treatment, do both change to the same extent?

The final decision of this question obviously rests in obtaining accurate measures for the rays of light and for the tithonic rays. It is the comparison of those measures which is to settle the point.

In this Journal (Phil. Mag., Dec. 1843) I have described an instrument, under the name of the tithonometer, which gives indications by the production of muriatic acid from the union of chlorine and hydrogen. This instrument is affected chiefly by the indigo rays, or more correctly speaking, by those rays which extend over the blue, indigo, and violet spaces of the spectrum, having their maximum in the indigo. It is important that the reader should keep this fact in mind.

Optical writers have been greatly embarrassed for want of a photometrical instrument which can measure the intensity of light: the chief difficulty in the way is the impossibility of contrasting together lights that differ in colour. By all it is admitted, that the eye is able to judge of the amount of illumination of white surfaces, or the depth of shadows within small limits of error, provided the rays compared are nearly of the same tint.

But in the discussion on which I am now entering this very difficulty is increased a hundred-fold. We are required to measure the intensity of light which has passed through all sorts of absorbent media, and therefore has become excessively coloured. How shall we compare together the rays which have gone through sulphocyanate of iron, and are of a deep blood-red, with those that have passed through sulphate of copper and are of a bright blue?

Nevertheless this problem is capable of a complete solution,

and a photometer can be obtained which gives results comparable with those of the tithonometer: such an instrument I have constructed; it is exceedingly simple, as the following considerations prove.

We are to remember that the tithonometer gives indications which are expressive of the intensity of the blue rays generally; the blue tithonic rays are the rays which it measures. In using the term *blue*, it will be understood to comprehend the blue, indigo and violet, or the more refrangible rays generally. It is obvious, therefore, that the photometer which is to be used with it must measure the same blue rays, or in other words, the tithonometer and the photometer must be affected by rays comprehended between the same limits of refrangibility.

This can be effected by interposing in the photometer some absorbent medium which will admit no rays to pass it, except such as are in the limits of refrangibility with which the tithonometer is engaged. It is fortunate, as I have found, that such a medium occurs in a solution of sulphate of copper and ammonia.

Let a wooden box, six inches long, two wide and two deep, be provided; in the centre of its top an aperture three quarters of an inch in diameter is to be made; the box must be blackened interiorly, and a rectangular prism of wood be placed in the box, with its right angle in such a position that its edge bisects as a diameter the circular aperture: over this wooden prism a piece of clean white paper should be pasted, care being taken that where it bends over the right angle of the prism it is folded sharp. So far the reader will recognise in this Ritchie's photometer, as described in the Annals of Philosophy. Upon the aperture in the top of the box a glass trough is placed; it is made by drilling a circular hole, an inch in diameter, in a piece of plate glass one-third of an inch thick, and then placing on each side of it a thin piece of plate glass. This forms a circular trough, in which a strong solution of sulphate of copper and ammonia may be inclosed; over the trough a conical tube six or eight inches long is placed, so that the eye may see distinctly through the aperture in the top of the box the disc of paper, and more especially its dividing diameter.

Two small lamps are then prepared, of such dimensions that when set opposite the open ends of the box their rays may illuminate the paper; they are supposed to be adjusted so as to shine with equal intensity.

On looking through the tube a circle of blue light is seen, and, if the lamps are shining equally, its two halves are equally bright. At the commencement of every experiment this pre-

liminary observation should be made, and, if necessary, the proper adjustments secured.

Suppose now it were required to know how much blue light is transmitted by a given solution. A trough is to be provided, which may be formed by drilling a hole two and a half inches in diameter through a thick piece of plate glass; on each side of this a thin piece of similar glass is laid, the trough having been filled with the solution under investigation. Troughs made in this manner never leak; they completely answer their purpose, and are easily washed and refilled.

Let the substance under trial be a concentrated solution of bichromate of potash. Having adjusted the lamps and filled the trough, set it before one of them, supporting it by a proper foot. On looking through the tube of the photometer, if the absorbent cell of the copper solution has been previously removed, the circle of light will be seen very brightly illuminated; one of its halves of a yellow tint, but not much less luminous than the other. The copper cell being now restored to its place, on looking again through the tube there is a striking contrast, one half of the circle is of a bright blue, but the other seems totally black : with solutions which cut off the blue rays less perfectly this blackness is of course less intense ; in these cases the lamps are to be moved into such positions that the two halves of the circle are equally illuminated, and its dividing diameter invisible. As the eye is not disturbed with any difference of colour, the observation can be made within small limits of error. The calculation of the relative intensities can then be made by the common photometrical law.

As this photometer is affected by rays of the same refrangibility as those which affect the tithonometer, it is clear that if it be the rays of light which are operative in the union of chlorine and hydrogen, the results given by the two instruments should correspond within certain small limits of error.

With respect to the tithonometer, I have improved this instrument considerably; by shading it with a glass case, which cuts off thermometric disturbance; by taking the observations through a small telescope, which avoids parallax; by having the scale moveable so as to slide along the tube; by making one charge of gas last for a great number of experiments; by completing the tithonization before commencing; by altering the position of the adjusting wire, so as to bring it nearly down to the end of the tube. The detail of these changes would, however, detain me now too long; they will be given at some suitable time hereafter.

I may, however, record as a striking fact, that so great is the sensitiveness of chlorine and hydrogen, that a mixture will actually explode by the rays of an electric spark. In this

Journal (Dec. 1843, p. 402) I have already stated that silent combination would take place under these circumstances, but more recently I have had instruments repeatedly destroyed by explosions resulting in that way.

It being understood that the indications of the photometer and the tithonometer correspond, that they are affected by rays of the same refrangibility, we are enabled to proceed to the direct solution of the question and the determination of M. Becquerel's hypothesis.

A transparent medium, which absorbs to a greater or lesser extent the more refrangible rays, being selected, it is required to determine whether, when a given ray has passed through it, the chemical effect diminishes as the intensity of the more refrangible rays of light diminishes. According to M. Becquerel, the effect should be in direct proportion to the quantity of light; if, for example, the ray lost one half of its blue light, the chemical effect should diminish one half, &c.

We require, therefore, two observations: first with the photometer, to ascertain how much of the light escapes the absorptive action of the medium under trial; second, with the tithonometer, to ascertain what quantity of the tithonic rays escapes absorption. If the whole effect is due to light, the two observations should give the same result.

Before giving the results, which I have obtained in a tabular form, in order that I may be clearly understood, I will give a particular example. I took some naphtha, the colour of which was slightly yellow, and placing it in the glass trough before described, proceeded to determine its relation for the more refrangible rays of light. This was done by adjusting the two lamps of the photometer till they coincided, then interposing the trough between one of the lamps and the end of the photometer. On looking through the tube a great diminution of the intensity of the light on the corresponding semicircle of paper was observed; the other lamp was now removed until the paper disc was uniformly illuminated; the distance of the two lamps from the centre of the box was now measured, they were respectively twelve and fifteen inches; but the intensity of the light is proportional to the square of the distance.

(1.) For the light in the unobstructed beam 225 or 100.

(2.) For the light in the absorbed beam ... 144 or 64.

Supposing now that the value of the unobstructed beam be represented by 100, and we calculate the amount of light which passes through the naphtha, we find it is represented by 64. Consequently, of every hundred rays of blue light which fell upon this naphtha, sixty-four escaped absorption.

The naphtha trough was now carried to the tithonometer; first, it was determined how many seconds it took a given

beam of light, coming from an Argand lamp, burning steadily, to move the index through one division.

(3.) For the tithonic ray of the unobstructed beam 31ˢ.

The trough was then interposed in the column of light, and the number of seconds required to make the index move through one division determined.

(4.) For the tithonic ray in the absorbed beam ... 65ˢ.

But as the lamp might have varied in intensity, or the tithonometer in sensitiveness, the first observation was repeated; it gave

(5.) For the tithonic ray of the unobstructed beam 31ˢ.

This process of repetition was uniformly resorted to, and the mean of the two taken. It may be proper to remark, that there was rarely any perceptible difference.

It follows, that a ray which could effect the union of a given quantity of chlorine and hydrogen in 31 seconds, required after passing through the naphtha 65 seconds.

Calculating on these principles how many of the tithonic rays passed through the naphtha, as before, we find,

(6.) For the tithonic rays in the unobstructed beam 100.

(7.) For the tithonic rays in the absorbed beam ... 48.

Now, comparing this result (6.) and (7.) with the result (1.) and (2.) for light, we find that the absorptive action of naphtha of a slightly yellow tint is very much greater for the tithonic rays than for the luminous rays.

Consequently it follows, that it is not the light comprehended between the extreme blue and extreme violet rays which brings about the union of chlorine and hydrogen, but another and invisible class of rays, which is absorbed by naphtha under a different law for that for its action on light.

The foregoing example shows the mode by which I have obtained the results of the following table:—

Table *showing the absorptive action of certain media for the luminous and tithonic rays.*

Name.	Number of rays which escaped absorption.	
	Tithonic rays.	Luminous rays.
Naphtha.....................	48	64
Sulphocyanate of iron ...	66	58
Nitrate of iron	53	64
Red prussiate of potash...	12	15
Turpentine.................	20	38
Copaiva balsam............	24	42
Iodide of starch	40	33
Stevens's blue ink.........	30	33
Litmus water..............	16	23

From this table, therefore, we gather that the chemical effect produced by a given ray has no relation to the quantity of light which is in it; that a satisfactory explanation of the phænomena can only be given by assuming the existence and presence of another agent besides the light, and to which agent the chemical effect is due; that media are known, which in their absorptive action bear relations which are totally different for these two agents; and, finally, that, as prismatic analysis has also previously shown, no explanation can be given of these results by imputing them to the agency of light, we are forced to admit the existence of another imponderable principle, the same as that which passes in these papers under the name of tithonic rays.

In addition to the results obtained from the foregoing quantitative experiments, there are other phænomena of a very novel and interesting kind, from which we may draw an argument of overwhelming force. The discussion of these I shall now take up.

Early during the last century the remarkable appearance of phosphorescence, excited in the Bolognean stone and calcined oyster-shells, attracted the attention of chemists. Dufay, in France, wrote several papers upon it; and the experiments of Wilson in England are perhaps as fine a specimen of philosophical investigation as those early times can furnish. These results, which few are now acquainted with, deserve to be republished.

To Becquerel we are indebted for one of the most remarkable discoveries in connexion with this subject. He found that the rays of an electric spark, which had passed through glass, no longer preserved the quality of exciting phosphorescence; but when they passed through quartz, they retained that power unimpaired. This result is very strikingly shown by placing a piece of colourless glass and a piece of quartz on a surface covered with sulphuret of lime (oyster-shells calcined with sulphur), and discharging a Leyden phial a little distance off. The sulphuret will glow as brilliantly on the part covered by the quartz as on the uncovered spaces, but under the glass it will remain dark.

Nevertheless, this same sulphuret, carried into the sunshine, phosphoresces powerfully *under glass,* apparently showing that there is a difference between the phosphorogenic emanation of the sun, for thus M. Becquerel terms it, and that of an electric spark.

On this radiation, as it comes from the sun, M. Becquerel has treated in his paper, of which a translation is given in

Taylor's Scientific Memoirs (vol. iii. part 12). He determines the place of the phosphorogenic rays in the spectrum, after they have passed through a glass prism, and shows that the fixed lines which occur in the chemical spectrum, occur also among these phosphorogenic rays.

In passing, I may mention that at the time I published my account of these fixed lines (Phil. Mag., May 1843), I had no idea that any other chemist had seen them. It of course soon appeared that M. Becquerel had some months previously given an account of them to the French Academy. My result was wholly independent and without any knowledge of his. On comparing the two papers, it will be seen that there is a strong coincidence, not only in the manner of the experiment, but even in the very phrases of description. It is this which has drawn these passing remarks from me. Men who are pursuing the same object, and using the same resources, will employ even words that are alike, though they speak languages that are different and live thousands of miles apart.

On examining the plate given in M. Becquerel's paper, I was struck with the close resemblance between the phosphorogenic spectrum and that tithonographic spectrum on iodide of silver of which Sir J. Herschel has given an elaborate account (Phil. Mag., Feb. 1843). As far as the eye could judge they seemed perfectly alike ; the tithonographic spectrum in question was obtained by me in Virginia. This coincidence was so striking that it appeared almost certain that the phosphorogenic emanations of Becquerel were the same as my tithonic rays : there was the upper spectrum commencing at the line G, and going beyond the furthest confines of the violet, exerting a positive action ; there was also the lower spectrum, commencing at the line F and going below the red ray, and exerting a negative action ; a phænomenon absolutely the same as that traced on the Daguerreotype plate.

The phosphorogenic rays that come from the sun have the same place in the spectrum, or are dispersed by the prism exactly in the same way as the tithonic rays. To all appearance these may be expressions for the same agent.

We must remember, however, that the phosphorogenic rays of the sun differ from those of an electric spark. Glass to the former is transparent, to the latter it is not.

Before, therefore, I can carry this argument to the point on which I design it to bear, it is necessary to ascertain the index of refraction of the rays of an electric spark to which glass is impervious.

This I proceeded to determine in the following way :—At the distance of six inches from the terminations of two blunt

wires, between which the spark from a Leyden phial was caused to pass, I placed a lens of quartz, the focus of which for parallel rays was six inches, and then intercepted the resulting beam by a diaphragm with a circular aperture in it, one-third of an inch in diameter. I had caused an equiangular prism of quartz to be cut and polished from a large and perfectly faultless rock crystal; it was cut transverse to the axis. This prism I placed in such a way that in dispersing the beam that came through the circular aperture I got rid of double refraction and obtained only one spectrum; this was received on a metal plate, which, having been washed over with gum water, and sulphuret of lime dusted on it, offered an uniform phosphorescent surface, which might be set in a vertical plane. *When the spark passed I saw that the plate was phosphorescing on those portions where the more refrangible rays had fallen.*

But the transient light of a Leyden spark did not last long enough, nor was the phosphorescence it produced powerful enough to enable me to conduct the experiment in a way entirely satisfactory. I resorted, therefore, to the brilliant light which is obtained when a piece of metal, or what is far better, the hard variety of carbon which is obtained from the interior of gas retorts, is lowered upon mercury entirely filling a very small open porcelain cup, and the continuous discharge of a voltaic battery passed. The battery used contained fifty pairs of Grove's cells, but a smaller number would probably have been amply sufficient. All the remainder of the arrangement was as just described.

As soon as the light was emitted, I marked on the sulphuret of lime the beginning of the red, the centre of the yellow, and the termination of the visible violet ray. Then, stopping the current, I examined on what parts the plate was phosphorescing. The commencement of the glow was between the indigo and the blue; towards the blue it extended far beyond the visible boundaries of the spectrum; I could not see any divisions or points of maxima on it. The surface of the plate shone all over, except in the region of the less refrangible rays, and there were the traces of the negative action which M. Becquerel has so well illustrated in the case of the solar emanations; rays which, however, were first observed in the last century.

It is necessary to remark, that the rays from the voltaic discharge resemble those from an electric spark in their inability to traverse glass. On this observation all the value of the foregoing experiment depends.

But it can nevertheless be easily proved, that although glass is impervious to the phosphorogenic emanation coming from

the voltaic deflagration of any metallic bodies, the observation applies to transient discharges only. A voltaic light, which lasts but a moment, fails to cause phosphorescence through glass in the same way that an electric spark does; but if the discharge is continued, the surface presently begins to glow; and if maintained for several minutes, it shines as brightly as though a piece of quartz had been used.

The inability of an electric spark to cause phosphorescence is connected with its transient duration. The voltaic light enables us at pleasure to imitate the effects of an electric spark or those of the sun.

The phosphorogenic rays, whether they originate in an electric spark or from the sun, occupying thus the same place in the spectrum, and even exhibiting the same peculiarities as the tithonic rays on iodide of silver, we have next to determine whether this is an apparent or a positive identity.

Prof. Henry of Princeton read a paper before the American Philosophical Society in May 1843, in which he discussed all the leading mechanical properties of the phosphorogenic rays, and among other important experiments made some with a view of determining this particular question. A Daguerreotype plate and some sulphuret of lime were simultaneously exposed to the sky; the plate was stained, but no effect was produced on the lime. A Daguerreotype plate and some sulphuret of lime were exposed to the light of an electric spark; the lime was observed to glow, but no impression was produced on the plate. When the plate was exposed to a succession of sparks for ten minutes, with a sheet of mica interposed, an impression was made. Lime exposed to the moon did not phosphoresce, but a sensitive plate under the circumstances is said to be stained. In view of these different facts, Prof. H. observes, "These experiments, although not sufficiently extensive, appear to indicate that the phosphorogenic emanation is distinct from the chemical, and that it exists in a much greater quantity in the electric spark than either the luminous or chemical radiation."

From Wilson's experiments, it appears that he was aware that when the phosphorescent surface is warmed, so as to hasten the disengagement of light, the moonbeams may be found to have left traces of action upon it; feeble it is true, but nevertheless very apparent. We have seen also that the peculiarity of an electric spark is due to its transient duration. Before therefore a final decision can be obtained on this point, we are required to examine the effect of the tithonic rays and phosphorogenic emanation, under circumstances which are precisely similar as to intensity and time.

For the transient rays of an electric spark, quartz is transparent and glass is nearly opake. Having prepared a bromo-iodized silver plate, so as to be exceedingly sensitive, I set in front of it, at the distance of about one-third of an inch, a disc of quartz and one of crown glass, of equal thickness; and between a pair of copper wires, the interval of which was three-eighths of an inch, I passed the spark of a Leyden phial fifteen times; the distance between this spark and the sensitive plate was about two inches. On mercurializing, the plate was deeply whitened all over, equally so through the glass, through the quartz, and on the uncovered spaces; but a spot of seal-ing-wax, which I had put on the glass, left its shadow on the plate beautifully depicted; as were also the edges of the glass and the quartz. The two discs overlapped one another to a certain extent, but the corresponding portion of the silver plate was as deeply stained there as anywhere else.

Next I put a surface of sulphuret of lime in the place of the Daguerreotype plate, everything remaining as before. On passing fifteen sparks the lime phosphoresced powerfully under the quartz, but not under the glass, so that the difference between its shadow and that of the spot of wax could not be distinctly seen.

For these reasons therefore I adopt the view expressed by Prof. Henry, that the phosphorogenic emanation and the ti-thonic rays are distinct. Under the same circumstances, glass to the one is transparent, to the other it is opake.

Now upon what sort of evidence is it that M. Melloni is universally admitted to have established the physical inde-pendence of light and heat? was it not by showing that rock-salt is perfectly transparent to calorific rays, that glass is much less so, that Rochelle salt, alum, and sulphate of copper are almost opake? It is surely impossible to confound the phos-phorogenic emanation of an electric spark with its rays of light; the latter pass perfectly through glass, the former do not. So far as the eye can distinguish, an electric spark, the rays of which have passed through glass, differs in no respect from one the rays of which are received directly into the eye. If we consider the constitution of such a ray, previous to and after its passage through glass, the eye can discover no differ-ence; but, as respects the phosphorogenic emanation, there was something existing in that ray at the first of these epochs which had ceased to exist in it at the second; a something not having the quality of communicating any impression to the organ of vision; and that which we cannot see, surely no man will acknowledge to be light.

But the reader may inquire, what has all this discussion of

the characters of the phosphorogenic emanation to do with the existence of the tithonic rays as a fourth imponderable? A few words will show. From these considerations and experiments we have arrived at the conclusion, that there exist in the beams of an electric spark *invisible rays, which are therefore totally distinct from light*. They occupy the same spectrum region as the tithonic rays which decompose iodide of silver; their leading character is, that glass which is transparent to the rays of light is opake to them.

But the admission of this fact breaks down at once the doctrine of a trinity of imponderables, and compels us to enlarge our list of those living forces of chemistry. The great obstacle which is in the way of admitting the tithonic rays as a fourth imponderable, is in the circumstance that it would impress a very serious change on that science, and apparently afford an argument of weight against the mathematical theory of light. I believe that some great generalization will hereafter prove that all these imponderables are modifications of one primordial principle. I also believe that some capital experiment will hereafter show that the forty different metals we are acquainted with are merely modifications of one or two more simple forms; but these are things that we are unable to deal with now; and viewing the experiments which have been made in the last few years, not as mathematicians, but as chemists, all men must acknowledge that our prevailing doctrines of the nature and number of the imponderables are liable, before long, to undergo a very serious modification.

The admission that the phosphorogenic emanation and light are principles differing from the tithonic rays and from each other, relieves us of much difficulty in increasing our list of imponderables. If these principles differ thus intrinsically from one another, the question comes home to us—what are they? If electricity and heat and light are three recognised imponderables, are not the tithonic rays a fourth, *and the phosphorogenic emanation a fifth*?

In view of this, I would suggest the propriety of ceasing to call these last by the epithet of *emanations*, and of giving them the more appropriate name of PHOSPHOROGENIC RAYS.

And now what appears to become of M. Becquerel's hypothesis, that all the different effects we have been considering are due to light, and are presented to us under different aspects because everything depends on the nature of the receiving surface; that it is the same principle which affects the eye as light, decomposes chloride of silver as a tithonic ray, and makes sulphuret of lime shine as a phosphorogenic ray; that the difference is not in the radiant principle, but in the surface

on which it is received? To go no further in a discussion
which has already extended this paper too much, if the agent
is the same in all cases, and the difference perceived is due to
the receiving surface, how is it that a ray of light which has
passed through a piece of transparent glass can no longer
excite phosphorescence in the sulphuret of lime? Can we
escape the conclusion that the ray has had something removed
from it, or has had some modification impressed on it, or, in
short, that something invisible to the eye has been taken away?

To my mind these considerations are conclusive, *and I
therefore regard the tithonic rays as constituting a fourth im-
ponderable, and the phosphorogenic rays as a fifth.*

University of New York,
    April 12, 1844.

XVI. *Remarks on the Review of a work entitled,* " An Inquiry
into the Nature of the Simple Bodies of Chemistry," *in-
serted in the Philosophical Magazine for April* 1844. *By*
DAVID LOW, *Esq., F.R.S.E., Professor of Agriculture in the
University of Edinburgh.*

To Sir David Brewster, K.H., &c. &c. &c.
DEAR SIR,

ALTHOUGH I have not the pleasure of a personal ac-
quaintance with you, I will venture to address you as if
I had, on the subject on which I am now to trouble you.

I send you a little work published about six months ago,
together with the April Number of the Philosophical Magazine,
in which a criticism on the work appears. I call it a criticism,
but you will perceive that it is nothing but an attack, and a
very virulent one, on the author.

An opinion entertained by many chemists, it is well known,
is, that the simple bodies of chemistry, so called, are not
really simple, or that they can only be termed simple with re-
lation to the present state of our knowledge and our means of
analysis. I have ventured to enunciate the proposition some-
what more precisely, and endeavoured to prove that these
bodies cannot be separated in their nature and functions from
the great class of bodies which we know to be compound.
Thus chlorine cannot be separated as one of a distinct order
of natural bodies from cyanogen, with which it exhibits the
closest parallelism in its chemical actions and relations.

To establish and illustrate my argument, I have ventured
to inquire whether we cannot reasonably suppose the simple
bodies, so called, to be resolvable into other bodies of the
same class. I assume, in the first place, that all these bodies
may be resolved into three, hydrogen, carbon and oxygen,

thus adopting the mode of reasoning, not certainly the best, but the most available in the absence of experimental proof, of assuming the premises, and proving or disproving them by the results arrived at.   To this end I take a review of the fifty-five bodies termed simple, endeavouring to trace their relations with one another, and with the bodies which we have determined by experiment to be compound.

But one or all of the three assumed roots, namely hydrogen, carbon and oxygen, may be compound.   I first make the sup- position that one of them is compound, namely oxygen, be- cause while the atomic weight of oxygen allows us to suppose that the other bodies may be resolved into it, the atomic weight of these other bodies does not allow us to suppose them resolvable into oxygen.   This hypothesis, like the first, is to be tested by the results arrived at.   Sir Humphry Davy was led, though by a different train of reasoning, to a very similar conclusion.   He supposed that all the simple bodies might be resolvable into two, hydrogen and some unknown base : this base my argument leads me to infer is carbon.

But hydrogen and carbon may one or both be compound. I endeavour to show that in this case we must derive these bodies from molecules higher in the ascending order than those of known forms of matter, leading to a conclusion in ac- cordance with a doctrine favoured by many eminent metaphy- sicians regarding the nature of matter.   It was merely neces- sary to reach this stage of generalization without following it to its results, because the only proposition that I had to esta- blish was, that the undecomposed bodies of chemistry were not simple but compound, and in no sense distinct in their molecular constitution from the bodies which experiment had proved to be compound.

Your kindness will excuse me for entering into these details. I merely wish to explain to you that the subject falls fairly within the range of free discussion, and that my argument, whether well or ill conducted, is a simple and natural one ; and from the work itself, if you will take the trouble to peruse it, you will judge how far the opprobrious epithets applied to me personally are justified, either by the subject of my in- quiry, or the manner of treating it.

I am accused of ignorance of facts, of never having made a chemical experiment, of impertinence, of stating what is mon- strous, and of "passing the sentence of banishment," to use the writer's own words, against the opinion of Sir Humphry Davy on the subject of chemical elements, and so forth ; and the writer winds up by expressing his "strongest and well- grounded hope" that he will never look upon the like of me

again. In one case only he seems to direct attention to the matter rather than to the author, though it has no bearing upon the argument. In his last paragraph but one he points out an inaccuracy. Any one who would read a page of the work would see that the mistake was one of inadvertence, and no more to be founded upon than an error of the press. This strange production was only brought under my notice within these two or three days. I shall have an opportunity soon of extending my work in preparation for a foreign edition, and of answering all fair objections; but it is impossible, you will see, to answer mere ribaldry and personal abuse.

Now, my dear Sir, your high and honourable name is placed in connexion with the work in which this article appears. Such a name is an assurance to the world that the work will be a faithful record of the progress of science, and that it will not be abused to unworthy ends; I therefore confidently appeal to you to procure me redress for the injury which has been sought to be done me, and which can only be through the medium of the work itself. To any fair criticism I could have no right to object, but the article in question is in no sense, as you will see, a criticism, but a mere libel under the guise of a review.

It has been suggested to me that it has been written by some concealed enemy, who has abused the privilege of being a contributor. I am unwilling to believe that any one could have been so ungenerous. If it has been written by the editor, under the opinion that it is more easy to demolish an author than his arguments, I hope he will, on reflection, see that he has probably misconceived my argument, and which, if it is good for anything, cannot be stifled by the means of attack which he employs.

Craigleith House, by Edinburgh,   I am, &c.,
        June 5, 1844.   David Low.

NOTE.

That blame may not attach where it is not due, I avow myself the author of the so-styled " libel under the guise of a review," and am responsible for the " ribaldry and personal abuse " which it is alleged to contain.

That the review proceeded from no enmity, either open or concealed, on my part, must be granted when I assert, that I knew not of the existence of Prof. Low till I saw his work; this confession he will of course consider a humiliating one, as arguing myself unknown.

When leisure will permit me, I intend to offer a few observations on the Professor's communication.

July 10, 1844.   Richard Phillips.

XVII. *Reply to* Professor Draper's *Letter, in the preceding Number of the Philosophical · Magazine, on a work entitled* "Researches on Light." *By* ROBERT HUNT, *Secretary to the Royal Cornwall Polytechnic Society.*

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

IT is, I assure you, with many feelings of regret that I find myself called upon to reply to a letter from Prof. Draper on my recent publication, "Researches on Light," which appears in the last Number of the Philosophical Magazine.

Of honest criticism, however severe, I should not complain. I have committed my book to the public; by its merits let it be judged, and by its honesty of purpose I am prepared to stand or fall in the estimation of the scientific world.

Dr. Draper is evidently offended because his name does not appear sufficiently often in my pages. It would be a difficult thing to persuade me that I was bound to bow down and worship every idol which self had set up, or to convince me that any man, even though a Professor, was the best judge of the value of his own labours.

My object has been, as Dr. Draper quotes, "to give to *every one* his full share in those discoveries which have *accelerated the advancement of the art.*" He then complains "there is not one word in allusion to the application of the Daguerreotype to the taking of portraits from the life, &c., and which Mr. Hunt knows belongs to me." I am quite aware that there is not a word on the subject; I never intended that there should be. The object of the "Researches on Light" is to show the remarkable powers exerted by the solar radiations, and not to deal with the mechanical arrangements of cameras. I believe there is not a single improvement in the chemical combinations used as the sensitive agents which I have not particularly described. In my "Popular Treatise on the Art of Photography," to which this matter belonged, I have made acknowledged quotations from Dr. Draper's paper which appeared in the Philosophical Magazine for September 1840. On referring to that paper it will be found that no alteration even is suggested in the preparation of the plates; but, taking advantage of principles *first pointed out by my particular friend Mr. Towson, in your Magazine for November* 1839, Dr. Draper succeeded in taking portraits from the life.

Dr. Draper then says, "there is scarcely an experiment of mine which is not either misrepresented or misunderstood." I deny the fact. If, however, it can be shown that misrepresentations of any kind exist in the volume, I pledge myself to give them the most public correction possible.

Dr. Draper next implies that the art of photography is more

indebted to him than I have allowed. I am not aware that he has devised a single photographic process; I cannot discover that he has even improved one.

Dr. Draper's inquiries into the condition of the solar rays have been extensive, and these are distinctly noticed in the "Researches." I am now, in justice to myself and others, compelled to state that I avoided any reference to Dr. Draper's paper on Tithonographic Spectra, published in one of your journals, to which I cannot at this moment refer, because nearly the whole of the prismatic analyses therein described had been long previously published by Sir John Herschel in the Transactions of the Royal Society, with which memoir Dr. Draper was evidently familiar, but which he avoided mentioning. Some other of the "nine-tenths" of the "*facts*" published by Dr. Draper, which he says "are not so much as alluded to," are in the same position as the above; and having in your Journal shown the doubtful character of the rest, and the absurdity of the Professor's conclusions[*], I leave it for your readers to decide whether I have not chosen the safest course in omitting them, when their insertion would have tended to involve an intricate subject in error.

Dr. Draper claims for himself the discovery "of a fourth imponderable," and then continues, "*Yet Mr. Hunt, in this book, actually appropriates it to himself, and gives the fourth imponderable the name of* ENERGIA." Now if your readers, instead of commencing their perusal at "that chapter which commences on the 259th page," will begin at the 243rd page, I think they will find I have given Dr. Draper credit for all that he has done. I am not inclined to dispute "the right of proprietorship over that capital fact," although I can prove that previously to 1842 I suggested, in lectures at the Polytechnic Hall, the probability of the discovery of a new element in the solar rays. Dr. Draper has done no more. I think the following passages will show no desire on my part to appropriate the ideas of others.

"Sir John Herschel admits the three classes of phænomena to be distinct from each other, and all the researches of Becquerel, of Moser, of Draper and others have led to the same conclusion.

      *   *   *   *   *

"*Dr. Draper, as I have previously mentioned, has proposed a name which is so purely imaginary and inexpressive that it can never be generally adopted.*"—Researches, p. 266.

Dr. Draper has proposed TITHONICITY as the name for this suspected element, from some fancied analogy between the

[*] Phil. Mag., April 1843, p. 270. In this paper it will be seen that due credit is given to Dr. Draper for all his tithonic researches.

loves of two poetical creations of the ancients, and the action of this element on material bodies. I have objected to such a name, and I have merely suggested the advantages of another term. "Might we not then with strict propriety, regarding this principle as the sun's *energetic* power, working in and producing change in bodies, adopt such a term as ENERGEMA, energy; or, slightly modifying it, use such a coinage as ENER-GIA, which is capable of being readily adapted to all the combinations we are likely to require?"—*Researches,* p. 269.

Two terms are suggested; it is questionable after all if either of them will be adopted ; but this rests with the public.

I am sorry to occupy so large a portion of your valuable pages with a matter so entirely personal; a few words more, and I have done.

It may appear to many, upon reading Dr. Draper's letter, that he has made a discovery of a new element, of which discovery I have endeavoured to deprive him. It should be distinctly understood *that no new imponderable element has really been discovered.* The solar beam has been long known to produce certain effects; these have been distinguished as light, heat, and chemical power. The idea generally received has been, that this chemical power was dependent upon some peculiar condition of light; but, since the beautiful discoveries which photography has led us to have been made, an idea has been floating in the minds of philosophers that this chemical power was something which was intimately associated with, but which was not, light. In the *Comptes Rendus* for 1840, M. Biot suggests this distinction, and objects to the name of "photogenic drawings" as confounding "two species of phæ-nomena." The question has since that time been taken up by many others; Messrs. E. Becquerel and Melloni both contending that the effects produced are due to light. Now Dr. Draper entertains the idea that the power in the sun's rays producing chemical change is a new element, and he is at once desirous of fixing the idea by giving it a name. This idea is as much Biot's as Draper's. I believe it will eventually be proved that the idea is a correct one. I have objected, and do object, to Dr. Draper's name. I have proposed two others, "Helio-plaston" and "Energia," as being more expressive. Another was suggested to me which is probably more expressive than either, it is "Metamorphia:" in speaking of the *metamorphic rays* we should at once express the peculiar features of the phænomena in question. It rests with the public to decide upon the epithet.

Notwithstanding the tone of Dr. Draper's animadversions, I can assure you and him, that I shall ever feel more pleasure in corroborating his statements than in correcting his errors.

I am not ambitious to deprive Dr. Draper of any of the fame that he covets, but I hope to be allowed to pursue my investigations, which I enjoy from their own exceeding great reward, without disturbance. I shall always feel it my duty to express my opinions on matters which have come under my investigation with that freedom which I hope others will use towards me.   I remain, Gentlemen,

Yours faithfully and obediently,

Falmouth, July 15, 1844.   Robert Hunt.

XVIII. *On the occurrence of Fluorine in Recent as well as in Fossil Bones.* By Charles Daubeny, *M.D., F.R.S.*[*]

HAVING in the course of the preceding spring paid a visit to the deposit of compact phosphorite which occurs in the province of Estremadura in Spain, I was subsequently led to examine into the chemical constitution of the mineral which forms the prevailing ingredient of the vein to which my inquiries had been directed.

The results of my examination have already been reported in the memoir communicated by my fellow-traveller Captain Widdrington, R.N. and myself to the Geological Society with respect to the rock in question, and read at their meeting on the 17th of January last; from which it will be seen, that the mineral, although, not being crystallized, it is somewhat variable in its composition, yet when selected as pure as possible, contains as much as 81 per cent. of phosphate of lime, and 14 per cent. of fluoride of calcium, the remainder appearing to consist of silica and peroxide of iron[†].

[*] Communicated by the Chemical Society; having been read March 18, 1844. On the same subject see Mr. Middleton's paper in our last Number, p. 14.

[†] The phosphate of lime I have since satisfied myself to possess the same composition as apatite, viz. $PO_5 + 3CaO$; and this is confirmed by Mr. Middleton and Mr. D. Campbell, in the laboratory of Professor Graham, who were kind enough each to undertake an independent analysis of the mineral. They likewise agree with me, and with each other, in their estimation of the amount of fluoride of calcium, determined by the loss occasioned on the addition of sulphuric acid to the pounded specimen, from which loss the proportion of fluorine was calculated. I should however remark that this quantity is double that of the largest amount reported by Rose to be present in the crystallized apatites which he examined (see Thomson's Mineralogy, vol. i. p. 125); and that I have never succeeded in obtaining more than 8·8 grains per cent. of fluoride of calcium by the direct method, in which the fluorine was expelled by heat and sulphuric acid from the phosphorite contained in a platina still, and where, after passing it through a silver vessel kept at 500°, in which any earthy matter carried over with the vapour might be deposited, it was finally received into a glass vessel containing caustic ammonia. As sulphate and fluate of ammonia could alone be formed, I estimated the amount of fluorine by adding chloride of calcium, and separating the resulting sulphate of lime from the fluoride of

The conclusion arrived at with respect to the compact form of mineral phosphate of lime occurring in the above locality, coupled with the reports of other chemists to the same effect relative to crystallized apatite, naturally led me to speculate as to the final causes of the apparently constant association of fluoride of calcium with earthy phosphates amongst the older materials of the globe, and to ask myself, whether it might not be possible, that fluorine, as well as phosphorus, fulfilled some hitherto unexplained office, in the œconomy of those organic beings, for the sake of which such mineral matters may be conjectured to have been treasured up in the rock formations from the beginning of time.

These reflections brought to my mind the researches of Morichini and of Berzelius with respect to the existence of fluorine in bones, seeing that the latter, according to the concurrent testimony of both these philosophers, appear to

calcium by repeated washings with water. I apprehend, however, that in this method the whole of the fluorine may not have been driven over, in spite of all the pains bestowed upon the process.

I am desirous of appending the analysis given by Messrs. Middleton and Campbell of the mineral, as though they differ, as might be expected, somewhat from my own in the entire quantity of phosphate of lime present in the specimens they examined, they confirm nevertheless my statement as to the relation both between the phosphoric acid and the lime, and between the phosphate of lime and fluoride of calcium present.

The portion examined by Mr. Middleton was in the form of a brown earth, containing nodules of a pure white, corresponding probably with the original condition of the formation generally. The results of analysis were as follow :—

Phosphate of lime	73·66
Fluoride of calcium	15·10
Phosphate of iron	4·26
Peroxide of iron	1·16
Matter insoluble in acids	3·95
Loss on heating to low redness after thorough drying	·78
Loss in the process	1·09
	100·00

The constitution of the phosphate seems to be $CaF + 2 (3CaO\ PO_5)$.

Analysis by Mr. Campbell.

1. 6·89 grains of a mixed substance, as received from Dr. Daubeny :—

Phosphate of lime ($3CaO\ PO_5$) .	4·78 =	69·38
Phosphate of iron	·70 =	10·16
Fluoride of calcium	1·01 =	14·66
Peroxide of iron	·02 =	·29
Insoluble matter in acids . . .	·38 =	5·51
	6·89 =	100·00

2. Analysis of white crystalline solid mass; 11·00 grains gave,—

Phosphate of lime ($3CaO\ PO_5$) .	8·68 =	78·90
Fluoride of calcium	·63 =	14·82
Matter insoluble in acids . . .	·66 =	6·00
Loss in experiment	·03 =	·28
	11·00 =	100·00

contain as a constant ingredient a minute quantity of fluoride
of calcium, inasmuch as its presence is vouched for by them,
in recent as well as in fossil bones, and in the teeth of mam-
malia, as well as in other parts of their osseous structure.

Here however I was compelled to pause, by observing the
contrary statements put forth by other able chemists relative
to this point; Fourcroy and Vauquelin having, previously to
the researches of Berzelius, denied the existence of fluorine
in recent bones, and Dr. Rees having, subsequently to them,
in a memoir drawn up under a full knowledge of what had
been done before, arrived at a conclusion equally opposed to
that of the Swedish philosopher*; one too which has been
since corroborated in a communication relative to the com-
position of bones made to the French Institute by Messrs.
Girardin and Preisser of Rouen, and lately published in the
Comptes Rendus†.

As however none of these gentlemen appear to dispute that
fluorine does occur *in fossil bones* generally, the conclusion
they have arrived at leaves the subject, it must be confessed,
encumbered with greater difficulties than before, for as all
sound chemical analogies stand opposed to the admission of
the idea, that fluorine can have been generated from the other
constituents, during any process of decay or alteration that
might have occurred in it, during the ages that had elapsed
since it formed a part of the living structure, we should be
driven to the belief, that the fluoride of calcium contained in
bone had filtered in from without,—a conjecture which, al-
though perfectly plausible, if the occurrence of this ingredient
had been casual, or had been limited to bones found in rocks
of a certain age or composition, seemed rather a violent one,
when extended to those of all ages and formations, being
scarcely reconcileable with the rarity of the mineral itself in
the waters of springs, and its sparing solubility in most re-
agents.

These difficulties that occurred to my mind, no less than
the weight I attached to the positive testimony of the great
Swedish chemist in favour of the existence of fluorine in re-
cent bones, induced me to consider, whether it might not be
possible that certain circumstances had operated in the mode
of conducting the experiment, by which the presence of fluo-
rine in the hands of the chemists who adopt the opposite con-
clusion escaped detection.

And on further investigation it appeared to me, that two
ingredients naturally present in recent bones might have in-
terfered with the result in the instances alluded to.

        * See Phil. Mag. S. 3. vol. xv. p. 558.—Edit.
        † Ibid. S. 3. vol. xxiv. p. 154.—Edit.

The first of these is animal matter, which, owing to the strong affinity it possesses for fluorine, may arrest its escape, and thus prevent it from coming into contact with the glass; the second, salts containing any volatilizable ingredient, such as the carbonic or muriatic acids, which would be disengaged by the same agent by which the fluorine was set at liberty, and which, escaping in a rapid current, might carry the latter along with them, before it could have time to exert any sensible action upon the glass suspended over it. Accordingly I found, that whilst one-tenth of a grain of fluor spar mixed with more than 100 grains of any earthy mineral, occasioned under the action of sulphuric acid an easily discernible, though faint corrosion on the exposed parts of the glass, the same quantity produced no effect whatever, when mixed with 5 per cent. of carbonate of lime, or with a little gelatine; and that half a grain of fluor spar and the earth, when mixed with gelatine, caused a trace on glass not much more distinct than that occasioned by one-tenth of a grain without this admixture.

In testing, therefore, the bones and teeth which I had obtained for examination, I did not choose to content myself with merely adding sulphuric acid to the pulverized specimen, but I began by burning off all the animal matter; and then, finding that the carbonic acid still in part remained, I dissolved the earthy residuum in muriatic acid, and threw down by means of caustic ammonia the earthy phosphates.

The latter, after being well washed and dried, were treated with concentrated sulphuric acid in a platina crucible, covered over by a plate of glass, shielded, except on the parts intended to be acted upon, by a coating of wax, but no artificial heat was applied, as the sulphuric acid, by its action upon the phosphate, raised the temperature sufficiently to expel whatever fluoric acid might be present in the specimen.

The glass was allowed to remain as a cover to the platina crucible for at least two hours, and in order to ensure the condensation upon it of the hydrofluoric vapour, a rim of wax was placed round the margin of the upper surface of the glass, by means of which a small portion of water might be kept the whole time in contact with it, so as to maintain a suitably low temperature.

That these precautions were not unnecessary I satisfied myself, by observing the difference in the degree of corrosion produced by a fossil bone given me by Dr. Buckland from the cave of Kirkdale in Yorkshire, when thus purified from the animal matter of which its long interment had not yet deprived it, as well as of its carbonic acid, as compared with the

same, when treated with sulphuric acid without having un-
dergone such a preparation.

In proof of this I submit to the inspection of members
specimens No. 3 and No. 4; the one showing the glass cor-
roded by a Kirkdale bone deprived of its animal matter and
carbonic acid; the latter, by one retaining both. Operating
in this manner, I have succeeded in engraving upon glass,
not only by means of fossil bones, from Stonesfield, from
Montmartre, from the cave of Kirkdale in Yorkshire, and
from that of Gailenreuth in Franconia, specimens of all
which were supplied me by Dr. Buckland; but likewise with
the bone of some quadruped that had been lying for a long,
but unknown time, exposed to the weather in the soil of our
Botanic Garden; with the vertebra of an ox recently killed;
with the tibia of a human subject from an anatomical cabinet
at Oxford; with the teeth of an ox just killed, and with human
teeth of recent date.   The markings differ widely in the de-
gree of their distinctness, and are in some instances so faint
as hardly to be discerned except by daylight; but I have
convinced myself that they cannot be attributed to the disen-
gagement of phosphoric acid, as the same glass was in no de-
gree affected by the fumes proceeding from the action of sul-
phuric acid upon pure phosphate of lime, where the acid had
been derived from the direct combustion of phosphorus, nor,
for a long time at least, by the vapour of free phosphoric acid
exposed to a heat sufficiently great to fuse and partially to volatilize it.
volatilize it.

Nor was it dependent on any peculiarity in the nature of
the glass, for plate glass was corroded in the same manner as
the crown glass more usually employed.

By the oldest and most fossilized specimens the glass seemed
undoubtedly to be most deeply etched; yet even here there
occurred exceptions, for the marks caused by a bear's bone
taken from Gailenreuth are the faintest in the whole series,
and were produced only after a long exposure to the acid va-
pours, two previous trials having proved unsuccessful; whilst,
on the other hand, the tibia of a human subject gave indica-
tions almost as distinct as any even of the fossil bones ope-
rated upon.

It would doubtless have more satisfactory if I could
have stated the proportion of fluorine in these samples of bones
and teeth, as well as the fact of its actual presence, and like-
wise if I had extended my examination over a larger number
of specimens; but I have been compelled to postpone the
former part of the inquiry until I could obtain an apparatus
suitable for the purpose, and doubted when my time would

permit me to carry further the present investigation, if, in order to give my results in a state of greater completeness, I neglected the present opportunity of communicating them*. The only criterion, therefore, I am at present enabled to offer as to the proportion of fluorine in the bones examined, is a comparison of the depth and distinctness of the marks pro- duced by the latter, with those caused by a certain known amount of fluor spar, mixed with a weight of phosphate of lime or other earthy material equal to that present in the bones operated upon. Judging by this rough mode of measurement, it would appear, that in several instances the faintness of the marks shows a smaller quantity of fluorine to have been pre- sent in the specimen, than would have been contained in a mixture of one-tenth of a grain of fluor spar added to 100 grains of phosphate of lime.

The existence of fluoric acid, as a constant, or at least a common ingredient in bones of all ages, would seem *à priori* to be much more probable, than its absence in recent bones would be, if its normal presence in fossil ones be admitted, for we can readily understand its finding its way into the ani- mal structure through the medium of plants, which may im- bibe it along with those phosphates with which it is so generally associated. Indeed it seems so likely, that those vegetables at least that contain much phosphate of lime should possess a trace of it, that I am at this very time examining the ashes of barley with reference to the latter point†.

The greater distinctness of the marks produced by the fos- sil bones acted upon than by the recent ones may be more difficult of explanation; but before it is urged as an objection against the view taken, it should be determined, whether the difference may not arise from the removal of the greater part of the animal matter from the fossil bone, owing to its long interment in the earth. Of the six specimens of fossil and recent bones of which I made a rough analysis, that from Stonesfield, which was the oldest of any, having been im-

---

* I have since, by the aid of the apparatus described in the former note, attempted to estimate the amount of fluorine in the fossil bone from Stones- field, and in the recent human bone from an anatomical cabinet. The former afforded 8·7 grains per cent. of fluoride of calcium, the latter only 2·0, results which will at least indicate the *relative*, if not the *absolute* quantity of fluorine present.

† I have since ascertained that no sensible action is exerted on glass by heating with sulphuric acid the earthy phosphates present in 12 lbs. of barley. Sprengel, I find, had already suggested the probable occurrence of fluorine in plants, but conceives that it exists in such a state of combi- nation, as causes it to be dissipated by the heat necessary for expelling the carbonaceous matter, and therefore cannot be detected in the ordinary method.

bedded in a secondary rock belonging to the oolite formation, lost, by exposure to a heat of 212°, 4·2 per cent.; by a further heat of about 500°, 5·0 per cent. more, and by increasing the temperature to a red heat only 1·8 per cent. in addition, the latter probably representing very nearly the amount of animal matter remaining, the two former numbers the water retained within the bone.

Proceeding upon the same data, the bone from the tertiary rocks of the Paris basin, next in the order of antiquity, would contain 10 per cent. of water, 2 of animal matter; the bone from Gailenreuth, water 13·9, animal matter 5·0; that from Kirkdale 12·5 water, 11 animal matter; whereas the recent bone picked up in the Botanic Garden contained, even when dry externally, about 30 per cent. of water and 11 animal matter; and the human tibia, which had been kept in an anatomical cabinet for a certain time, gave out 23 per cent. of water and 17 of animal matter.

It may also be suggested, as a possible explanation, that the fluoride of calcium distributed through the mass has in the course of time become collected into little nuclei in certain parts of the bone, and for this reason may allow of a more ready disengagement from it of the fluoric acid which it contains as an ingredient.

That a certain alteration in the arrangement of the earthy particles of a bone does occasionally take place after its deposition, is evidenced from the curious observations by Messrs. Girardin and Preisser, in the memoir which has been already referred to, as these gentlemen state, that the bone-earth phosphate appears in some instances to have separated into two distinct compounds, crystals of apatite being recognised by them in some of the fossil bones in their possession, which they conceive to have arisen from the segregation of the tribasic from the bibasic compound.

Will not this latter fact also help us towards an understanding of the function which fluate of lime may fulfil in the structure of bones, and likewise of the peculiar adaptation of the bone-earth phosphate to serve as its prevailing earthy ingredient?

It seems a general law in both kingdoms of organic nature, that crystallization should operate as a sort of antagonist force to the processes of assimilation, so that no material can be fitted to enter into the fabric of a living body, between whose particles the natural force of polarity operates with all its energy. Hence, according to Dr. Prout, the use of the infinitesimal portions of foreign inorganic matter interposed between the particles of most bodies which form the constituents of vegetable or animal organization; and although it

may be true, as has been suggested by Von Buch, that the very prismatic form which belongs to phosphate of lime as a mineral species, adapts it for the fibrous structure of bone better than other earthy compounds in which the axis of crystallization is equal in both directions, yet even in this case the tendency to arrange itself according to the laws which regulate inert matter might operate too powerfully, were it not diminished by the association in equal atomic weights of the two phosphates, each of which possesses a polarity in some degree differing from the other, and consequently to a certain extent counteracts the disposition in the particles of the other to assume a determinate arrangement.

If there be any truth in these speculations, is it not also conceivable, that the interposition of a mineral matter, like fluor spar, whose particles crystallize in quite another manner, that is in cubes, may cooperate on the same principle, in imparting that freedom of motion to the particles of the prevailing constituent of bones, by which it is rendered more pliant to the purposes of the animal economy, more obedient to the laws of life, more ready in short to insinuate itself into the pores, so as to form the coats of those delicate capillary canals of which the osseous structure appears to consist?

XIX. On a Spathic Carbonate of Iron.
By Mr. John Thomas Way[*].

THE mineral which forms the subject of the present notice was put into my hands by Professor Graham. It would seem that its appearance had caused it to be mistaken for zinc blende, which indeed it somewhat resembles in its colour and lustre. Upon examination however it was found to contain no zinc, but to be essentially a carbonate of the protoxide of iron with hydrated peroxide of iron.

The mineral is massive, of a brownish-black colour, pretty compact structure, and exhibiting, when broken, crystalline, generally rhomboidal and curved facets of a somewhat pearly lustre. It is not homogeneous, some portions of it, when crushed in an agate mortar, giving a powder of a much whiter colour than others; none of it is magnetic.

Its specific gravity is 3·747.

This mineral yields nothing to water, but (with the exception of a mere trace of silica) is entirely soluble with effervescence in acids. Nitric and sulphuric acids dissolve it in the cold, but more readily with the assistance of heat. Hydro-

chloric acid appears to act upon it with more energy than either sulphuric or nitric acid.

When analysed, the mineral was found to contain carbonic acid, protoxide and peroxide of iron, lime and water.

In order to obtain it as homogeneous as possible for quantitative analysis, considerable portions of the mineral, which is not harder than marble, were reduced to powder and well-mixed.

The results of two analyses subjoined, however, show that its composition is not quite uniform.

	I.	II.	Mean.
Carbonic acid . . .	36·13	36·03	36·08
Protoxide of iron . .	51·33	50·18	50·75
Peroxide of iron . .	8·26	9·60	8·93
Lime	4·75	3·74	4·24
Water	1·02	1·14	1·08
	101·49	100·69	101·08

The excess in the analyses appears to be due to an imperfect mixture of the different parts of the mineral, several portions having been taken for the various operations.

The water is as nearly as possible in the proportion to form protohydrate of the peroxide ($Fe_2O_3 + HO$), and there is little doubt exists in that condition.

The composition of the mineral may therefore be thus expressed:—

Carbonate of protoxide of iron . .	83·60
Carbonate of lime	7·47
Hydrated peroxide of iron	10·01
	101·08

In examining the composition of this substance, considered as a variety of the spathic carbonate of iron, the first peculiarity worthy of attention is the absence of manganese.

It would appear from all recorded analyses of the spathic carbonate of iron, and also of carbonate of iron as existing in clay-iron-stone, that manganese is a usual ingredient of both varieties. Dr. Thomson of Glasgow describes, in his ' Outlines of Mineralogy,' a brown mineral under the name of " Hydrous Carbonate of Iron," which appears from the analysis he supplies to be of similar constitution to that under notice,—consisting of carbonate of the protoxide of iron with hydrated peroxide of iron, but differing in containing so much as 4·75 per cent. of protoxide of manganese.

In four out of eight analyses by Dr. Colquhoun of clay-iron-stone from the coal beds in the neighbourhood of Glasgow, given in the same work, manganese is mentioned, al-

though in much smaller quantity; and the analyses by Berthier of nineteen varieties of iron ore from the coal beds of France exhibit the uniform presence of the oxide of this metal in a proportion varying from 0·5 to 4 per cent.

The absence of manganese is perhaps, however, less interesting in a scientific than in an œconomical point of view, and can easily be conceived in a crystallized mineral, which appears to have undergone so little alteration.

Its freedom from silica (with the exception of a mere trace not mentioned in the analyses, and existing as grains of quartz and not in combination), from alumina, magnesia and phosphoric acid, are also matters more affecting the value of this ore as a source of iron than its interest as a subject of scientific inquiry.

The action of acids upon the mineral is peculiar, and throws some light on its constitution and the probable circumstances of its formation. When treated in the state of fine powder with diluted hydrochloric acid it is gradually dissolved with effervescence, but the action is by no means rapid; if heat be applied considerably more effect is produced, but not such as we are accustomed to witness in the solution of carbonates.

If the acid be poured off after a short digestion with heat upon the powdered mineral, the residue undissolved is in the state of a white crystalline powder, which dissolves very slowly even in concentrated, and scarcely at all in dilute mineral or in vegetable acids.

Upon examination, this crystalline carbonate was seen under the microscope to consist of well-defined perfectly transparent crystals, having the rhomboidal form of calc spar. Notwithstanding the prolonged action of acids to which the mineral had been subjected, the crystals were found to retain a considerable quantity of lime. It was thought advisable therefore to subject the substance to analysis. The result was:—

Protoxide of iron	56·14
Carbonic acid . . . . . . : .	39·03
Lime	5·04
	100·21

Its composition is therefore

Carbonate of protoxide of iron .	91·24
Carbonate of lime.	8·97
	100·21

It will be seen that the carbonate of lime is here in larger proportion than in the original mineral, which should be the case if the digestion in acids removes only or principally the peroxide of iron.

There can be little doubt that the deposition of carbonate of lime had occurred in the crystals simultaneously with that of carbonate of iron, a circumstance of easy explanation from the isomorphous relations of iron and lime in their carbonates.

The simplest method of dissolving up the peroxide of iron so as to obtain this pure carbonate of iron, appears to be to digest the powdered mineral in strong hydrochloric acid till all the black matter is removed, which occurs in a few minutes, to wash and dry by a heat of 212°.

The power which carbonate of iron in this state opposes to the action of acids is well illustrated in another instance, in which masses of the size of split peas were introduced into commercial hydrochloric acid diluted with an equal bulk of water, a state most favourable to the solution of a carbonate. After digestion for a night, the acid remaining in excess, the carbonate was found undissolved, the only effect being the removal in solution of the hydrated peroxide and consequent disintegration of the mineral. Some of the carbonate had undoubtedly dissolved, but the greater part escaped.

This separation of the constituents of the substance under examination is a very distinct denial to the supposition that they exist in a state of combination, and indicates at the same time a very considerable resistance to external agencies. It comes then to be an interesting mineralogical inquiry in what way the peroxide of iron found its way into the mineral. In the description of the "hydrous carbonate of iron," Dr. Thomson states his belief that the peroxide had originally existed in the state of carbonate of the protoxide, but that by exposure to the weather this had lost carbonic acid and become peroxide.

I might perhaps be allowed to go a step further, and suggest the possibility that some of the lime which exists in the mineral might, in solution in carbonic acid water, have penetrated it by infiltration, and that the excess of carbonic acid would dissolve the carbonate of iron, and afterwards, upon evaporation, leave it exposed as hydrated carbonate—a state most favourable to atmospheric agency.

The value of this mineral, however, in an œconomical point of view, constitutes unquestionably its chief interest, and on this part of the subject it is to be regretted that only vague information can be afforded.

The mineral came from Hull; the only information that accompanied it is, that "it forms a vein of from 4 to 10 fathoms in thickness by upwards of 100 fathoms in width, within about 15 miles of coal and railway." This of course would afford an enormous supply.

By the analysis it will be observed that the total amount of iron as carbonate and peroxide is equivalent to about 45 per cent. Comparing this with Mushet's black band (the richest ore which is worked in England), we have, it is true, no great preponderance in favour of the spathic carbonate, except in the purity of the ore. In comparison, however, with the ores usually employed in this country it possesses great advantages.

Of 7 analyses of clay iron-stone from the Glasgow fields the average in iron is 30 per cent.; showing an advantage of 50 per cent. on the product to be obtained from this mineral.

This, although a vast recommendation, is perhaps the least that can be urged in favour of this valuable ore. The ease with which it would be worked is another subject of considerable importance. A simple reduction with carbonaceous matter is probably all that would be required, there being little or no impurities which would call for a flux to remove them.

But the remarkable purity of the mineral is that which as a source of iron would principally constitute its value, and would fit it eminently for the production of the best varieties of that substance, and would enable it to rival successfully the Swedish ore in this especial application of iron.

I regret exceedingly my inability to supply more tangible information as to the extent and locality of this ore, and its relations to the coal strata, which must materially affect its value; but repeated attempts to obtain this information have proved abortive, I presume, from the existence of a jealousy on the part of the proprietors to divulge it.

It is well however that the attention of mineralogists should be drawn to the subject, as it is by no means improbable that spathic iron ore may exist in other localities to an equally large extent.

XX. *Remarks on* Sir John F. W. Herschel's *Observations on the Entrance Passages in the Pyramids of Gizeh.* By a Correspondent.

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

THE arguments brought forward by Sir J. Herschel in his "Observations on the Entrance Passages in the Pyramids" (reprinted in No. 162 of the Phil. Mag., S. 3. vol. xxiv. p. 481, from Colonel Vyse's work on the Operations carried on at the Pyramids of Gizeh in 1837), prove pretty satisfactorily that the inclined passages there alluded to were designed for astronomical purposes, though the principal and almost fatal objection to such an hypothesis, namely, that the entrances to these tunnels or passages were carefully and se-

curely closed by their constructors, has been allowed to remain unanswered.

On repeating the calculations given at the end of the paper I have found two or three trifling typographical errors, which are unworthy of notice; but there is one of another kind which requires correction, since it is an error in computation and has considerable effect on the final result. The value of P α being 24° 51′ 10″ and that of a' 21° 25′ 48″, a'' must evidently be 3° 25′ 22″, instead of 2° 25′ 22″. The last step of the calculation then becomes

$$
\begin{aligned}
&\text{Log sec } 21° \ 25′ \ 48″ = 0\text{·}03111 \\
&\text{Log cos } 21 \ \ 28 \ \ 50 = 9\text{·}96874 \\
&\text{Log cos } \ \ 3 \ \ 25 \ \ 22 = 9\text{·}99922 \\
&\text{Log cos P}'\alpha = \overline{9\text{·}99907} \\
&\phantom{\text{Log cos }} P'\alpha = \overline{3° \ 45′}
\end{aligned}
$$

Assuming the latitude to be 30°, this corrected north polar distance of α Draconis gives 26° 15′ for the altitude of that star at its lower culmination, and on taking the mean of Colonel Vyse's measures of the angles of the descending passages in the three principal pyramids we find the result to be 26° 13′, a coincidence so remarkable that there can be little doubt of the correctness of the date assigned for the erection of these vast structures; and it is even more probable than the illustrious writer of the " Observations " supposed, that when first built, the passages respecting which so much has been written were " directly pointed at α Draconis, at its inferior culmination."

<div style="text-align:right">

I am, Gentlemen,
Your obedient Servant,

</div>

July 3, 1844.   R. H.

XXI. *Observations on Colour Blindness, or insensibility to the impressions of certain colours.* By Sir DAVID BREWSTER, *K.H., D.C.L., V.P.R.S. Edin.*

IN a very able and interesting memoir on Colour Blindness, which I have just received from my distinguished friend Prof. Wartmann of Lausanne*, I observe some mistakes respecting my opinions on this subject, which I find it necessary to correct; and I do this with the more satisfaction, as they are not of a controversial character, and as it will give me an opportunity of gratifying the reader with an account of the leading results to which he has been led. The passage in

---

* A translation of this Memoir will appear in the 13th Part of Taylor's Scientific Memoirs, to be published in the course of next month.

Prof. Wartmann's memoir in which he has mistaken my opinion, is the following:—

"Tous les physiciens savent que les limites des sons perceptibles varient d'une oreille à l'autre; c'est ce que Wollaston et Chladni ont irrécusablement démontré. Sir D. Brewster* suppose, par analogie, que dans le cas de Daltonisme l'œil n'est pas impressionné par les couleurs de l'une des extremités du spectre. L'insensibilité de quelques yeux aux faibles impressions lumineuses s'explique, dit il, en ce que la rétine par son organisation naturelle, ou par quelque cause accidentelle, peut être moins délicate ou moins susceptible des impressions de la lumière chez une personne que chez une autre, sans que les facultés visuelles en soient affaiblies d'une manière concomitante†."

The opinion contained in the preceding extract was given in December 1821, and is more an illustration from analogy, than an explanation of the phænomenon. It differs, indeed, very little from that previously given by Dr. Thomas Young, who attributes colour blindness to "the absence or paralysis of those fibres of the retina which are calculated to perceive red;" and it differs nearly as little from the opinion afterwards given by Sir John Herschel‡, Dr. Elliotson, and Prof. Wartmann himself, that colour blindness arises "*from a defect in the sensorium.*" Now Prof. Wartmann observes, in reference to my explanation as given above, "*il me semble que cette théorie manque des preuves.*" True,—it not only wants proofs, but it is incapable of being proved, and so is the theory of a *defect in the sensorium*; and so is Dr. Young's theory of a want of proper fibres, or of a paralysis of existing fibres. All these theories, indeed, are mere conjectures, and Dr. Dalton's hypothesis of a blue vitreous humour is the only one capable of being proved or disproved.

As I know nothing about the *sensorium*, or about its connexion with, or mode of operation upon, the nerves of sensation, I shall leave the discussion of the merits of the new theory to those who feel themselves qualified for so arduous a task; I shall content myself with bringing forward some important and, I believe, new facts, which will at least illustrate, if they do not confirm, the opinion which I have expressed respecting the cause of colour blindness.

In the sentence immediately preceding the one above quoted by Prof. Wartmann, I state that "I have lately ascertained that some eyes which perform all the functions of vision *in the*

---

* Edin. Phil. Journal, vol. vi. p. 141, Jan. 1822.

† *Mémoire sur le Daltonisme*, par Elie Wartmann, Prof. de Physique à l'Académie de Lausanne, &c. &c. Genève 1844, 54 pp.

‡ Treatise on Light, Art. 507.

most perfect manner are *insensible to certain impressions of highly attenuated light* which are *quite perceptible* to other eyes." Now it was a very natural conclusion that this insensibility might be greater for some colours than for others, and as Dr. Wollaston had just published his discovery of a similar phænomenon in reference to the ear*, and had ascribed it to a difference in the state of the tympanum, I conceived that an analogous view might be taken of colour blindness. Perhaps Dr. Wollaston was too bold in ascribing *cricket deafness*, as it may be called, to the state of the tympanum, and not to a *defect in the sensorium*, and I also too bold in pursuing the analogy from the *tympanum* to the *retina*. Dr. Wollaston has not mentioned any case, though I have no doubt there are many, in which *cricket deafness* is confined to *one ear*. Although my own hearing is perfect, and each ear equally acute for all ordinary sounds, yet one of them is absolutely deaf to the chirp of the cricket, while the other hears it distinctly. Now it will not, I presume, be maintained that there is a sensorium for each ear. In like manner, and I have no hesitation in predicting it, there may be found persons whose colour blindness is confined to one eye, or at least is greater in the one eye than in the other. Nor is this wholly a conjecture from analogy, for my own right eye, though not a better one than the left, which has no defect whatever, is more sensible to red light than the left eye.

But there are still stronger points to be adduced in favour of these views. I have proved from numerous experiments, that when the retina is rendered partially insensible by the action of light upon any one part of it, *it first becomes insensible to red light*; and hence we have a distinct reason why *red-colour blindness* is the general character of the defect under consideration; and I am persuaded that any defect of sensibility produced by the action of light or by any other cause, will, if carefully examined, be found to be a maximum with *red* light.

In experiments of this kind, in which what we may call *artificial colour blindness* is produced, the intensity of the light is always diminished; but it remains to be determined by accurate observation whether the *red* end of the spectrum (for example) when seen *yellow* by an eye defective in its judgment of colours, is *brighter* or more *obscure* than it would have been had no such defect existed. I am persuaded, from many observations I have made, though I do not consider them as decisive of the question, that the object is seen more obscure, and that certain of the rays emanating from it are not appreciated by the nervous membrane. If, on the other hand, every

[* Dr. Wollaston's paper on this subject was reprinted in Phil. Mag. S. 1. vol. lvii. p. 187.]

ray from the red object is efficacious, and the only effect is the substitution of a sensation of *yellow* or *green* in place of *red*, then we might expect that the object would appear brighter, in so far as a *yellow* sensation produced by a given number of red rays should be brighter than a *red* sensation produced by the same number.

After some observations on the foregoing quotation, Prof. Wartmann proceeds thus : " Plus tard Sir D. Brewster parait avoir changé d'opinion*. En partant de la *supposition* que la choroide est essentielle à la vision, il *conjecture* que l'invisibilité de la couleur rouge chez les Daltoniens est due à ce, que la rétine elle-même a une teinte bleu, de telle sorte que la lumière étant privée de ses rayons rouges par le pouvoir absorbant de cette membrane, l'impression colorée sur la choroide sera dénuée de rouge."

Prof. Wartmann is quite mistaken in thinking that I have changed my opinion ; the passage which he quotes as mine is from an *anonymous* notice on the subject, and he was not, therefore, entitled to attach my name to an opinion which I did not avow. It is quite true that I wrote the anonymous notice, but the notice itself, had it been *fully quoted*, would have shown what was my *real opinion*, as distinguished from a *conjecture* founded upon a possible and expressed supposition. The passage in the anonymous notice quoted by Prof. Wartmann is as follows :—" Dr. Brewster conceives that the eye is in those cases (of colour blindness) insensible to the colour at one end of the spectrum, just as the ear of certain persons has been proved by Dr. Wollaston to be insensible to sounds at one extremity of the scale of musical notes, while it is perfectly sensible to all other sounds." Now this is my opinion to which, though writing anonymously, I attach my name; and in the very next sentence I throw out the conjecture, quoted by Prof. Wartmann, from which I withheld my name ; and yet he supposes that the conjecture is mine, and attaches my name to it, while he overlooks my real opinion, bearing my name, in the very sentence which precedes the conjecture.

But, after all, the conjecture was not an idle one. During the dissection of many hundred eyes, I observed in several cases that the *vitreous humour* was of a decided *greenish-blue* colour, and in other cases that the *retina* had a marked *French gray* or *pale blue* tint, which decidedly absorbed red light. I knew that in cases of colour blindness the vitreous humour was not *blue*, or even *greenish-blue*, as Dr. Dalton conjectured ; but I could not assert that in the same cases the retina might not be blue, and hence I was led to hazard the idea of a blue

* Edinburgh Journal of Science, No. 7. or vol. iv. p. 86, Jan. 1826.

retina as one which might be admissible as a cause of colour blindness; but *only on the supposition* that the choroid coat should prove to be the seat of vision.

In treating of the various forms of colour blindness, Prof. Wartmann considers the *greatness of their number* as not giving support to the constitution of the solar spectrum of *equal and coincident spectra* of the three primitive colours. " C'est ce nombre si grand des variétés du Daltonisme qui *me semble une objection* contre la preuve que Sir David Brewster a cru trouver dans cette anomalie pour appuyer sa théorie de trois couleurs élémentaires (Edin. Journ. of Science*, N. S., tom. v. p. 206; *Bibl. Univ.*, tom. l. p. 147). On ne peut pas dire que 'le fait physiologique, et le principe d'optique sur lequel il fonde son analyse du spectre sont parfaitement d'accord et se confirment mutuellement.'" What Prof. Wartmann calls my *theory of three elementary colours* is a *fact* as rigorously demonstrated as any physical truth can be ; but if he does not admit it as *true* over the whole length of the spectrum, he cannot avoid, if he makes the experiments, admitting it as true over the greater part of it ; and this is all that is necessary for my present argument.

In the year 1822, in a paper published in the Edinburgh Transactions, vol. ix. p. 433, I first established the fact, that in part of the spectrum different colours were superposed. In 1827, Sir John Herschel, whose experiments had previously led him to an analogous result, remarks, in his Essay on Light, "that this doctrine is not without its objections; one of the most formidable of which may be drawn from the various affections of vision," namely colour blindness, which he goes on to describe. My principal memoir on the triple spectrum, founded on an immense number of physical experiments, did not appear till 1831 ; and though I should never have dreamt of bringing to my aid, in such an inquiry, a *physiological fact*, such as that of colour blindness, I found it necessary to remove, if possible, the objection drawn from it by so distinguished a philosopher as Sir John Herschel. My views needed no other support than what they derived from direct experiments with absorbing media applied to spectra, either entire or cut up into bands by the interference of polarized and of common light ; but when I believed that I had removed the force of the objection, I was entitled to say that the views in question derived that kind of support which must always be gained by the removal of an objection.

* The original memoir, of which an abridgement only appeared in the works here cited, was published in the Edinburgh Transactions, vol. xii. p. 135.

As I do not understand how this negative support can be affected by the great number of varieties of colour blindness, I cannot reply to Prof. Wartmann's statement, but I shall en- deavour to explain my own view of the matter. According to the doctrine of the triple spectrum, the red space consists of *red, yellow* and *blue* light, the *red* predominating and the *blue* being extremely feeble. Now the late Mr. Troughton, whose colour blindness was examined by Sir John Herschel and my- self, on separate occasions, saw this *red space, yellow.* Hence, according to my views, he saw a space containing much *yellow* and little *blue,* the *red* light being as it were absorbed, in conse- quence of the nervous membrane being insensible to its action. If this be the case, there must have been a diminution of light in the red space seen by Mr. Troughton, and I am persuaded, from the experiments * I made upon his eyes, that this was the case; but whether it was to the extent of the total defalcation of the red rays I will not venture to assert. But it is not ne- cessary that it should be so; *the defective perception of red light may be accompanied with a more acute perception of the other colours,* in a manner analogous to what takes place in the chemical spectrum, where the removal of the red rays produces an increased action of the rays which are left; the luminous rays may act upon the nervous membrane with negative and positive influences; and this view of the subject is greatly fa- voured by the results which I published in 1832, relative to the increase in the intensity of light by physiological action †. In many cases too of imperfect vision, I have long been of opinion that the retina receives a more powerful luminous im- pression from yellow light than from the pure white light, of which this yellow forms but a part; and I have therefore re- commended the use of yellow glasses as best fitted to excite a torpid retina.

But whether these views be sound or not, I cannot conceive how an *infinite variety,* even of cases of colour blindness, can have any bearing whatever on the doctrine of the triple spec- trum. In order to be a case of colour blindness, one or more tints must be wanting or unseen, and there is no tint what- ever which cannot be produced by the supposed absorption of individual rays of the compound spectrum.

We regret much that Prof. Wartmann has continued the offensive name of *Daltonism* in his memoir on colour blind- ness, especially as he agrees with Dr. Whewell in opinion, that no person wishes to be immortalized by his imperfections. We cannot but regard it as degrading to the venerable name

---

* He confounded with green leaves red petals, which were far more lu- minous than the leaves.

† Lond. and Edinb. Phil. Mag., vol. i. p. 173, 1832; vol. iv. p. 241, 1834.

which it misapplies, and as one of the worst examples of vicious nomenclature. The name was first imposed by Prof. Prevost of Geneva, and Prof. Wartmann has continued it on the sole ground that it has for forty years been employed in oral instruction in the academy of that city. Dr. Whewell's name of *Idioptcy* or *Idiopts* is clearly inadmissible, and we cannot speak more favourably of the name *Achromatopsie*, used by Purkinje; *Akyanoblepsie*, used by Goethe; *Chromatopseudopsy*, used by Sommer and Szokalspi; and last and worst of all, *Chromatometablepsie*! We have used the word *Colour blindness*, because it indicates simply blindness to one or more colours*.

The following is Prof. Wartmann's recapitulation of the principal results of his able and interesting memoir:—

1. *Daltonism* has not been studied by the ancients.
2. It has been found only in individuals of the white race.
3. There are very many varieties of the affection, from persons who see only black and white to those who in candlelight confound approximating shades of blue and green.
4. There are more Daltonians than is generally believed.
5. The female sex furnishes a very small proportion.
6. In certain cases they may be recognised by external signs.
7. There are as many Daltonians with *blue* as with *black* eyes.
8. Daltonism is not always hereditary.
9. It does not always affect the males of the same family.
10. It does not always commence at birth.
11. The Daltonians do not judge as we do of complementary colours.
12. Several of them are not sensible to the least refrangible rays.
13. They see, as we do, the lines in the spectrum discovered by Fraunhofer.
14. They do not judge as we do of the contrast of colours.
15. Daltonism does not arise from a vicious conformation of the eye, or any coloration of the humours or the retina.
16. We may alter the state of Daltonism by very simple means.
17. Daltonism has its origin in a defect of the sensorium.

In treating of the differences of colour seen by each eye, Prof. Wartmann ascribes to M. Quetelet, or rather quotes from him, Dr. Smith's of Fochabers beautiful experiment, originally described and fully discussed in this Journal (Phil. Mag. S. 3. vol. i. p. 249, 1832, and vol. ii. p. 168); and in his observations

[* A paper on a case of colour blindness by the late Mr. G. Harvey, was reprinted from the Edinburgh Transactions, in Phil. Mag. S. 1. vol. lxviii. p. 205.]

on the theory of accidental colours he does not seem to be aware
of the facts and discussions in this Journal, vol. xv. p. 441,
Dec. 1839.

St. Leonard's College, St. Andrews,
        July 10, 1844.

XXII. *Proceedings of Learned Societies.*
ROYAL SOCIETY.

[Continued from vol. xxiv. p. 468.]

May 16. A PAPER was read, entitled, "On the Measurement
        of Distances by the Telescope." By Edmund Bow-
man, Esq., C.E. Communicated by S. Hunter Christie, Esq., Sec.
R.S.

The method proposed by the author for determining distances by
means of a telescope, consists in placing, at the spot of which the
distance is required, a graduated staff, and observing the number of
its divisions comprehended in the field of the telescope, or included
between fixed points in a diaphragm placed in the focus of the eye-
glass. He finds that the number of these divisions, apparent in the
field of view, are directly as the distance of the staff, plus a certain
constant, which depends on the construction of the instrument. The
author investigates the value of this constant, and illustrates the prac-
tical applications of his method, which he thinks might be employed
with great advantage in surveying, when, from irregularities of
ground or difficulties of access, the direct measurement by the chain
would be inconvenient or impossible.

2. "An Account of some Experiments exhibiting new instances of
the Absorbing Power of Streams; with a few remarks on the Pul-
sation of Jets." By Mr. G. Robinson. Communicated by W.
Bowman, Esq., F.R.S.

The experiments of which an account is given in this paper
illustrate the absorbing power of a stream of fluid, whether issuing
from the open orifice of a reservoir, or flowing through rigid tubes.
The effects of this power are seen in the position of the fluid con-
tained in a vertical tube open at both ends, placed within the reser-
voir, and of which one end is brought within the influence of the
effluent stream : and also, when one end of a bent tube is brought
into the stream issuing from the open orifice of a reservoir, while
the other end is immersed in a coloured fluid. The author accounts
for the intermitting or pulsatory character of the jet issuing from an
open horizontal pipe, having a small hole on the upper side, by the
introduction of air, which, accumulating from time to time, forms a
bubble, which when it has attained a certain size, occasions an
obstruction to the free passage of the liquid, until the obstacle is
overcome by increased pressure from behind, and the jet then re-
sumes its former velocity. These changes occurring periodically,
give rise to the appearance of pulsation which is observed in these
circumstances.

May 23.—1. "Meteorological Register kept at the Master At-

tendant's Office, Trincomalee, between the 1st of September, 1843, and the 29th of February, 1844." By Joseph Higgs, Master Attendant. Communicated by the Lords Commissioners of the Admiralty.

2. "On the supposed Properties of the Electric and Magnetic Fluids." By W. F. Stevenson, Esq., F.R.S.

The author is of opinion that electricity is a single undecomposable fluid, and that the distinction usually made into vitreous and resinous, or positive and negative electricities, is derived altogether from the direction of its motion and the circumstances under which it is presented ; and, according as it is found on a conducting or non-conducting body, it is positive in the former case and negative in the latter. The quality of the electricity is, according to the author, modified by the form of the conducting body, which, when globular, opposes its escape; but, when pointed, facilitates its passage in a current. He considers the magnetic fluid as obeying the same law as the electric fluid, that is, moving in a current, which when aided, and not interrupted, will always be found positive, or having a north pole, at that end of the conductor or magnet where the fluid is escaping ; and negative, or with a southern polarity, at the opposite extremity.

3. "De l'Education des Animaux ; faisant suite à l'ouvrage publié en 1842, et qui a pour titre Essai sur l'Education des Animaux." Par St. Léonard de Lille, Membre de diverses Sociétés scientifiques, et de l'Athénée des Arts de Paris, et son Employé des Finances. Première partie de l'Institut et de l'Intelligence, Education et Civilisation. Communicated by J. F. Daniell, Esq., For. Sec. R.S.

The present paper purports to be the sequel of a work, already published by the author, on the subject of the education of animals. It is the first part only of the paper which is here presented, and contains preliminary observations on the nature of the inquiries which the author proposes to enter into in the subsequent parts. It is divided into three chapters ; the first comprising some general remarks on the objects to be attained in the education of animals, and some criticisms on the opinions of preceding writers relating to the subject ; the second treating principally of Instinct and its characteristic features, as contrasted with Intelligence and Reason ; and the third entering into various metaphysical disquisitions on the nature and peculiar sphere of action of the different intellectual faculties, both those which are common to man and the lower animals, and those which are peculiar to the former.

June 13.—1. "On the Action of the Sun's Rays on Lithic Acid." By John Davy, M.D., F.R.S. Lond. and Edinb.

The author, after adverting to the composition of guano, and remarking that its nitrogenous part differs from the urine of the seafowl, from which it is derived, chiefly in containing little or no lithate of ammonia, but a large proportion of the oxalate, describes an experiment made for the purpose of determining whether the oxalic acid existing in guano may not be formed from the lithic acid of the urine, in consequence of the operation of the sun's rays.

The result of a comparative trial made with the urine of the white-headed Sea-Eagle, in one instance kept in darkness, in the other exposed to bright sunshine for many successive days, afforded an answer in the affirmative. Whilst the urine of the Sea-Eagle, con-sisting chiefly of lithate of ammonia, kept ·in the dark, underwent no change, that exposed to light was materially changed, most of the lithate of ammonia had disappeared, its place was supplied by oxalate of ammonia, the peculiar odour of guano was acquired, and in part its brownish hue.

From considering the composition of the lithic and oxalic acids, the author infers, that in the conversion of the one into the other, oxygen is absorbed ; and, in confirmation, he mentions that although no change takes place when moist lithate of ammonia is exposed alone to a temperature of about $212°$, it is otherwise if so treated when mixed with black oxide of manganese, in which case oxalate of ammonia is formed, and also some brown colouring matter, not unlike that of guano ; and this colouring matter, he supposes, may appropriate to itself the excess of carbon and hydrogen, that is, such proportions of these substances in the lithic acid as are more than those required to form oxalic acid and ammonia.

Having always found in the South American guano more distinct traces of lithate of ammonia than in the African, he inquires whether the difference may not be owing to different states of atmosphere in the two regions ;—in the one, the clouded state of the air impeding the sun's rays ; in the other, the usually unclouded state interposing no obstacle to their full effect ? He inquires too, whether the circumstance of the comparatively rapid conversion of lithic acid into the oxalic under the influence of light, as witnessed in the experiment detailed, may not account for even recently formed guano being destitute of lithate of ammonia ; and he men-tions an example in point, namely, a specimen he had received from the island of Ichabor being found with a large quantity of oxalate of ammonia, to contain no lithate of ammonia,—a specimen described as " having been scraped off a rock, where it was in a thin layer, and much exposed to the sun."

2. " An Account of the Newtonian Dial presented to the Royal Society, in a letter to the President." By the Rev. Charles Turnor, F.R.S

The dial here described was taken down in the early part of the present year from the south wall of the Manor-house of Wools-thorpe, a hamlet to Colsterworth in the county of Lincoln, the birth-place of Newton. It was marked on a large stone at the angle of the building, and about six feet from the ground. The name of NEWTON, with the exception of the first two letters, which have been obliterated, are inscribed under the dial in wide and capital letters. The gnomon has disappeared many years ago.

3. " On the Non-coincidence of the Focus of the Photogenic Rays with that of the Visual Rays of the Solar Spectrum." By M. A. Claudet. Communicated by S. Hunter Christie, Esq., Sec. R.S., &c.

After detailing the difficulties he had met with in obtaining per-fect pictures when a lens, accurately corrected for spherical and chromatic aberration, was employed in the Daguerreotype process,

the author states that in order to obtain a clear and well-defined image of any object on the Daguerreotype plate, he generally found it necessary to adjust the focus on the ground glass by another ob- ject brought considerably nearer to the camera than the object whose picture was required. When this adjustment is made, he proceeds to apply the principle practically to the taking of portraits. He finds that in achromatic object-glasses the focus of photogenic action is not coincident with the visual focus; and the distance be- tween these two foci varies according to the nature of the combina- tion of the glasses, to their different dispersive powers, and to the degree of intensity of the light. By attention to these circumstances in accurately adjusting the Daguerreotype plate to the situation of the focus of the photogenic rays, the author has succeeded in ob- taining the most perfect delineations of objects*.

4. " Observations on some of the Nebulæ." By the Earl of Rosse, F.R.S.

The nebulæ of which an account is given in this paper, were ob- served with the speculum of three feet aperture described in the Philosophical Transactions for 1840: and the object of the observa- tions was rather to test its powers and to decide the merits of pro- gressive experiments than to seek for astronomical results. Sketches are given of the actual appearance of five of the nebulæ observed, namely those numbered 88, 81, 26, 29 and 47 in Sir John Herschel's catalogue. The author observes, in conclusion, that all that he has seen confirms the accuracy of Sir John Herschel's judgment in selecting the nebulæ which he places in the class designated as re- solvable; and that every increase of instrumental power still con- tinues to add to the number of the clusters at the expense of the nebulæ, properly so called. It would still, however, be unsafe, he further remarks, to conclude, that such will always be the case, and thence to draw the obvious inference that all nebulosity is but the glare of stars too remote to be separated by the utmost power of our instruments †.

June 20.—1. " On the Structure of the Ultimate Fibril of the Muscle of Animal Life." By Erasmus Wilson, Esq., Lecturer on Anatomy and Physiology in the Middlesex Hospital; in a Letter addressed to Peter Mark Roget, M.D., Sec. R.S. Communicated by Dr. Roget.

By resorting to peculiar methods of manipulation, and employing a microscope of more than ordinary power, the author, with the assistance of Mr. Lealand, has succeeded in discovering the real structure of the ultimate muscular fibril, in a specimen taken from the arm of a strong healthy man immediately after its amputation. He finds each fibril to be composed of minute cells, disposed in a linear series, flattened at their surfaces of apposition, and so com- pressed in the longitudinal direction as to leave no marginal in- dentation on the surface; thus constituting a uniform cylinder, divided into minute subdivisions by transverse septa, which are

* The necessity of the adjustment here alluded to was first pointed out by Mr. Towson, in 1839. See Phil. Mag. S. 3. vol. xv. p. 381.—EDIT.
† See Phil. Mag. S. 3. vol. xix. p. 588.—EDIT.

formed by the adherent surfaces of contiguous cells. The diameter of the fibril, in the state of relaxation, is the 20,000th part of an inch. The cells are filled with a transparent substance, to which the author gives the name of *Myoline*, and which differs in its re-fractive density in different cells. In four consecutive cells the myoline is of greater density than in the four succeeding cells, and this alternation is repeated throughout the whole course of the fibril. In consequence of all the fibrils composing the ultimate fasciculus having the same structure, and the cells, which are in lateral juxta-position, containing myoline of the same density, they act similarly on light, and the whole presents, to the eye of the microscopic ob-server, a succession of striæ or bands, dark and luminous alternately, and transverse to the direction of the fasciculus; an appearance which has been noticed by preceding observers, but of which the cause had not hitherto been ascertained. A dark stria may occa-sionally appear as a luminous one, and *vice versâ*, when viewed by light transmitted at different degrees of obliquity.

The structure here described, the author remarks, reduces the muscular fibre to the simple type of organization exhibited in the combination of a series of cells, associating it with other tissues of cell formation, and will probably, he thinks, open new sources of explanation of the immediate agency of muscular action, a power hitherto involved in the deepest mystery.

2. "On the Comparative Anatomy of the Thyroid Gland." By John Simon, Esq., Assistant Surgeon to King's College Hospital, and Demonstrator of Anatomy in King's College. Communicated by Joseph Henry Green, Esq., F.R.S.

The author, considering that the careful dissections of Meckel and Cuvier have fully established the universal existence of a thyroid gland in the whole of the class Mammalia, proceeds to consider the comparative anatomy of this organ in the remaining classes of ver-tebrated animals. His dissections of birds have included all the orders, and, in most instances, several families from each: he has never failed to find in them a thyroid gland, and, with the aid of the microscope, to recognise its peculiar structure; he presumes, there-fore, that it is universally present in that class of animals. He has also detected the presence of this organ in reptiles of every order; although generally either wholly overlooked by anatomists, or mis-taken for the thymus. Descriptions are here given of its appearance, position and structure in different families of Chelonia, Sauria, Ophidia and Batrachia. In the class of Fishes, it is by no means universally or even generally present. The author has found it in the carp, anableps, pike, exocetus, cod, haddock, whiting, eel, stur-geon, callorhynchus, shark and skate, and perhaps in the lamprey. On the other hand, it appears to be absent in the perch, mullet, gurnard, mackerel, tench, salmon, trout, herring, plaice, halibut, turbot, sole, cyclopterus, gymnotus and balistes.

The general conclusion which the author deduces from his re-searches is, that the distribution of the thyroid gland is regulated by a simple and uniform law; being dependent on the existence or non-

existence of another organ with which its presence alternates, and which, in many fishes, assumes the form of a minute supplementary gill, the vessels of which communicate, on the one hand, with the systemic veins about the base of the cranium, and on the other, by a single long trunk with the first branchial vein.

Although the thyroid gland occupies various situations in different animals, it always maintains an intimate relation with the vascular supply of the brain, and is always so nourished as to be capable of a greater or less nutrition according to the activity or repose of that nervous centre.

3. " On the Resolution of Numerical Equations." By Joseph Agar, M.D., Fellow of the Royal College of Physicians. Communicated by John Ayrton Paris, M.D., President of the College.

The object of this paper, which is purely analytical, is to explain a method of resolving numerical equations with real coefficients, which recommends itself by its simplicity and generality.

4. " On the Reproduction of lost parts in Myriapoda and Insecta." By George Newport, Esq., F.R.C.S., President of the Entomological Society of London, and Corresponding Member of the Philomathic Society of Paris. Communicated by P. M. Roget, M.D., Sec. R.S.

It has long been known that the limbs of Crustacea and Arachnida, accidentally lost or designedly removed, are, in course of time, replaced by the growth of new limbs ; and the same power of reproduction has been stated to have been observed in the Phasmæ, insects which undergo neither metamorphosis nor any change of habits. But whether such a power exists in those insects, such as the Lepidoptera, which undergo a complete metamorphosis, changing not only their form, but also their food and mode of life, in passing from the larva to the adult state, has been considered as very doubtful. The instances in which the reproduction of lost parts appeared to have occurred in some of the Myriapoda, were attributed to imperfect or arrested development. With a view to determine these unsettled points, the author commenced, in the summer of 1841 and 1842, a series of direct experiments on this subject in the Myriapoda ; and in the present summer he has extended them to the Lepidoptera. The results of his labours are given in the present memoir.

In some specimens of Iulus, from which he had removed the antennæ and some of the legs, the lost organs were found to be completely reproduced after the next change of integument ; differing from the original organs only in their smaller size, and the incomplete development of some of their minuter parts. The same results followed from similar experiments made on the Lithobris during the earlier periods of its growth. One individual of this genus, which had already acquired the tenth pair of legs, was by accident deprived of the eighth, ninth and tenth pair ; at the next change of skin it not only developed two additional pair of legs, but also reproduced the three pair which had been lost. Some time after this it again lost one of the legs of the twelfth pair ; a loss which was repaired

at the next change by the growth of a new leg, while those previously reproduced acquired an increase of size.

The first observation which led the author to believe that true insects might possess the power of reproducing lost parts, was that of
a specimen of Phasma in the collection at the British Muscum, in
which the right anterior leg had evidently been reproduced. He
then instituted a series of experiments on the larva of the *Vanessa
urticæ*, or common nettle butterfly, which belongs to the order
Lepidoptera, and undergoes complete metamorphosis. He removed
some of the true legs of the larva, sometimes in their tibial portion,
and sometimes at their base : in the first case, parts similar to those
removed were invariably reproduced in different states of development, and in the latter, entire new limbs were formed ; in some instances, at the second change of the larva, when it passed into the
pupa state ; but in two or three instances no reproduction took
place. At first view, this difference in the results might appear to
favour the opinion that this reproduction of limbs depends on the
existence of parts especially adapted to perform this function, and
which, in those experiments that had failed to exhibit the phenomenon, had been themselves removed. But the author found that
in every instance of the mutilations thus practised, the perfect insect possessed a coxa, or basilar part of the limb ; and this was the
case even in those in which a new organ was not reproduced. From
this fact, taken in conjunction with the formation of new entire limbs
in the Iulidæ after the removal of every portion of the previous
ones, the author infers that the power of reproduction resides in the
whole of the organized tissues.

The author found that each newly produced limb is, in every
case, composed of all its essential parts, namely coxa, femur, tibia,
tarsus and claw ; but its development is scarcely ever entirely normal, being either deficient in some of the tarsal joints, or irregular
in the development of its armature.

The following are the general conclusions which the author deduces from his investigations. Slight wounds in the larvæ of insects
always heal, except when the viscera have protruded, or excessive
hemorrhage has occurred : severe wounds, such as those attending
the excision of a limb, also frequently heal. It is when the wound
is in the line of action of the principal muscles of the body that protrusion of the viscera takes place. For the healing of wounds, the
first requisite is the arrest of the hemorrhage ; and this is effected,
as in the higher animals, by the coagulation of the blood, and the
formation of a clot ; and then a complete union of the separated
parts takes place beneath the eschar formed by the clot. After this
union, the reparation of the injury is commenced by a development,
from the injured surface, of parts corresponding to those that had
been removed. For the production of a new limb, one change of
skin, at least, is necessary. The healing of the wound after the
removal of a part, and the subsequent reproduction, although they
do not prevent, yet certainly retard the natural changes. Lastly,

the author has established the fact, that reproduction of lost parts takes place in metabolic as well as in the ametabolic articulata.

The paper is accompanied with drawings of reproduced parts.

5. " On the Changes of Temperature produced by the Rarefaction and Condensation of Air." By James Prescott Joule, Esq. Communicated by P. M. Roget, M.D., Sec. R.S.

In order to estimate with greater accuracy than has hitherto been done the quantities of heat evolved or absorbed during the condensation or rarefaction of atmospheric air, the author contrived an apparatus where both the condensing pump and the receiver were immersed in a large quantity of water, the changes in the temperature of which were ascertained by a thermometer of extreme sensibility. By comparing the amount of force expended in condensing air in the receiver with the quantity of heat evolved, after deducting that which was the effect of friction, it was found that a mechanical force, capable of raising 823 pounds to the height of one foot, must be applied in the condensation of air, in order to raise the temperature of one pound of water one degree of Fahrenheit's scale. In another experiment, when air condensed in one vessel was allowed to pass into another vessel from which the air had been exhausted, both vessels being immersed in a large receiver full of water, no change of temperature took place, no mechanical power having been developed. The author considers these results as strongly corroborating the dynamical theory of the nature of heat, in opposition to that which ascribes to it materiality ; but he reserves the further discussion of this question to a future communication, which he hopes soon to present to the Royal Society.

INSTITUTION OF CIVIL ENGINEERS.

June 11, 1844.—A paper on the purifying of coal gas, and the application of the products thereby obtained to agricultural and other purposes, by Mr. Angus Croll, was read. The process consists in passing the gas through a solution of sulphuric acid, of the strength of two and a half pounds of oil of vitriol to 100 gallons of water, and by a continuous supply of acid, so that the proper amount of free acid might be always kept in the vessel : the whole of the ammonia in the gas was abstracted, preventing the corrosive effect of this impurity of the fittings and meters through which it was transmitted, and rendering the gas capable of being used in dwelling-houses, and also enabling the gas companies to use dry lime, instead of wet lime purifiers without producing any nuisance on the opening of the vessels, by which a considerable saving was effected, while at the same time sulphate of ammonia of great purity was obtained, and of such a strength, that the evaporation of one gallon produced eighty ounces of this valuable salt, instead of fourteen ounces, which was the quantity rendered under the former process. The author concluded by showing the advantage to agriculture by the application of this produce ; he stated that various experiments upon an extensive scale had been tried with this manure with great success : one example will suffice for giving an idea of its powers.

One half of a wheat field was manured with sulphate of ammonia, at the rate of $1\frac{1}{4}$ cwt. to the acre, and at a cost of $1l.\ 2s.$, the other half with the ordinary manure; the latter produced only $23\frac{3}{4}$ bushels, but the former under the treatment of sulphate of ammonia produced $32\frac{3}{4}$ bushels. In the discussion that ensued, in which Prof. Graham, Mr. Cooper and many members took part, the advantages of the system were confirmed, and the necessity for its extension insisted upon. The various modes of purifying gas, and the value of the products obtained for agricultural purposes, were canvassed at length. It was stated that seeds steeped for forty hours in a solution of one pound of sulphate of ammonia to one gallon of water, sown in unmanured land, produced a heavy crop, and remained green during a dry season, when every other kind of vegetation became yellow and withered. Another remarkable feature was, that faded flowers, when plunged in a weak solution of sulphate of ammonia, were in a short time restored, and that plants watered with it attained extraordinary health and beauty. The great loss resulting from the leakage of the gas through the joints and the pores of the cast iron pipes, was incidentally mentioned, and it was stated that in some instances it had amounted to from 25 to 75 per cent. of the total quantity produced.

XXIII. *Intelligence and Miscellaneous Articles.*

ANALYSIS OF A PORCELAIN CLAY DISCOVERED AT HOWTH, NEAR DUBLIN. BY ROBERT MALLET, ESQ.

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

I BEG to transmit you the results of my analysis of a porcelain clay discovered some years ago by me at Howth, near Dublin, and since extensively brought into use for the manufacture of crucibles. The clay is found upon the southern side of the peninsula of Howth, which consists principally of quartz rock; it exists in large concretionary masses or highly irregular beds, and appears to have reached its present position by the transport of water. It is found of every degree of fineness, from a coarse gritty mass of decomposing pebbles, with occasional large nodules of friable felspar, to that of an impalpable colourless clay, like that of Dorsetshire, known as pipeclay: this is soft, ductile, adheres to the tongue, and forms a strongly adhesive and plastic mass with water, capable of being moulded upon the potter's wheel into the finest forms.

It bakes perfectly white, or occasionally of the slightest possible rosy tint of white; some of the masses of this mineral are strongly discoloured by iron and manganese, and imbedded in the finer parts are occasionally found a few fragments of marine shells and bits of wood.

By washing with abundance of water, a fine quartzose sand is separable from even the finest portions of this clay; this sand is white, but water separates from it a little sand of a darker colour, like

common sea sand of the Dublin coasts, and a few microscopic flakes of mica.

A singular minute black worm is found in this clay, which may be worth the attention of naturalists.

The clay as dug out does not effervesce with acids, and is insoluble in them. It yields no soluble matter to water, and appears to contain no alkali in any specimens yet examined. I have, however, reason to think that the less fully decomposed portions of the clay may contain alkali in a soluble condition, and hence render the material valuable as a manure.

Some of the finest portion of the clay washed from the sand, and dried at a temperature of 212° Fahr., was found by me to have the following composition : the analysis having been conducted in the usual way and with the usual precautions, it does not seem necessary to detail its steps :—

Silica........	67·96
Alumina	23·20
Lime	3·23
Magnesia	0·63
Oxide of iron..	1·19
Water	2·80
	99·01

As no washing completely removes the presence of sand from this clay, which always feels gritty to a glass rod, and as it contains comminuted mica, it could not be expected that its analysis should present a precisely mineralogical result.

From the close analogy however which the above figures present to the composition of various felspathic rocks as analysed by Beudant, Berthier, &c., there can be little doubt but that the geologic origin of this clay is the decomposition of felspar, or other granitic allied minerals ; in fact the results approximate to the formula (taking the iron and manganese together)

$$Al + Si + Ca + (Mg + FeO) + HO,$$

or $\quad 3(Al + Si_3) + (Ca + Si_3) + ((Mg + FeO) + Si_3) + HO.$
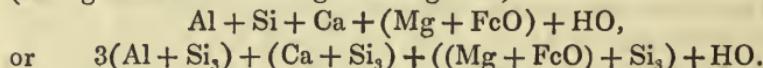
This clay is of very great œconomic value, and capable of being used for the manufacture of porcelain ; it has, however, hitherto only been brought into use for the manufacture of crucibles by the writer.

I am, Gentlemen, your obedient Servant,

Dublin, June 24, 1844. ROBERT MALLET.

EASTER DAY, 1845.

[In furtherance of the object stated in the following notice by Prof. De Morgan, we copy it from the Athenæum (No. 872).]

In an article which I have prepared for the next number of the Companion to the Almanac, I have discussed the reasons why Easter Day will next year fall, in apparent defiance of the Act of Parliament, upon the very day of the full moon. But as an accidental application which I have received (and I know that the Superintendent of the Nautical Almanac has received another from a different quarter)

makes me think that the computers of almanacs will be puzzled, and that the useless discussion of 1818 (when the discrepancy last occurred) will be revived, unless some one will forthwith state the reason of the difficulty, I request that you will publish some of the conclusions of the paper to which I refer, which will in due time appear in the work cited : and I hope that the daily papers will give additional currency to the same conclusions; and I should recommend that the same thing should be done in the almanacs.   The rule adopted in this country for finding Easter is that of the Roman Catholic Church, as established at what is called the reformation of the Calendar by Pope Gregory XIII. in 1582.   The authority for this rule is contained in the papal brief of March 1st, 1582, in which reference is made, for all explanations, to the then forthcoming work of the Jesuit Clavius, to whom both the adjustment and explanation of the Calendar had been entrusted.   The British parliament, in adopting the *rule* of Clavius, made two mistakes in the *explanation* of that rule.   Their explanation is, that Easter Sunday is the Sunday after the full moon which comes on or next after the 21st of March, and that if the full moon fall on a Sunday, Easter Sunday is the next Sunday.

The two mistakes are as follows :—

1. Instead of "full moon," they should have said "fourteenth day of the moon, the day of new moon being reckoned as the first." That Easter, as well as the Passover, was always regulated, not by the full moon, but by the fourteenth day of the moon, is of the utmost historical notoriety.   And Clavius says that "none but a few who fancy themselves sharp-sighted ever imagined that the fourteenth of the moon and the full moon were the same in the Church of God."

2. Instead of the "moon" of the heavens, they ought to have said the "moon of the calendar," which is a very different thing. The moon of the calendar is not even a mean, or uniformly moving moon to which astronomers refer the real moon; but differs from it intentionally and avowedly, by two classes of arbitrary alterations ; the first class intended for simplicity of calculation, the second for avoiding the possibility of the Christian Easter falling on the actual day of the Jewish Passover.

In the year 1845, the fourteenth day of the *calendar moon* falls on Saturday the 22nd of March, whence Easter is rightly made to be Sunday the 23rd, according to the law both of the Roman and English churches, though the English statute does not well explain its own method.   With regard to this country, it should be noticed that this statute enacts that Easter shall be kept by the "calendar, tables and rules" annexed to the act : and these agree with Clavius.

Of course any one is at liberty, as many did in 1818, to think that the statute should be altered ; and certainly, it would be worth while to avoid misconception by repealing the faulty definition, and substituting a better one, in the prayer-books of the established church. But if any one should wish to advocate the repeal of the rules, and the construction of new ones agreeable to the existing definition, and

astronomically true, he will perhaps pause when he finds that his own system would *sometimes* cause it to happen that St. Paul's Cathedral must keep Easter a week after Westminster Abbey, and would very frequently make a week's difference between the festivals of the colonies and the mother country.

University College, London,   Yours faithfully,
     July 11, 1844.   A. De Morgan.

EFFECTS OF NASCENT OXYGEN ON CERTAIN ORGANIC ALKALIES.
BY EUGENE MARCHAND.

When excess of peroxide of lead is boiled in a solution of sulphate of cinchonia, quina, morphia, narcotina, strychnia or brucia, and sulphuric acid is added drop by drop, until a portion of the filtered liquor, tested with ammonia, potash, or its carbonate, ceases to become turbid, there occurs a brisk effervescence of carbonic acid gas, and the liquor becomes of a deeper colour; if the operation be then stopped and any free acid which may exist in it be saturated with litharge, the solution evaporated to dryness, and the mass be treated with distilled water, and the solution obtained with sulphuretted hydrogen, which sometimes precipitates a little sulphuret of lead, and lastly the filtered liquor be evaporated to dryness, very distinct colouring matters are procured from each organic alkali; these colouring substances are the following:—

Cinchonetin; this is obtained by the process above described from cinchonia; its properties are that it is an uncrystallizable amorphous mass, of a very deep violet colour when viewed in mass, and yellowish red in very thin layers. It is deliquescent, and has a bitter taste. When attempts are made to incinerate it, it fuses, yields white vapours as cinchonia does, the odour of which is not at all ammoniacal; it inflames and burns with a sooty flame, and leaves a charcoal which it is very difficult to incinerate.

Cinchonetin dissolves more readily in boiling than in cold water; its best solvent is alcohol; æther does not act upon it; concentrated sulphuric acid dissolves it very easily and becomes of a red colour, but without apparently altering it; if this solution be treated with water, it assumes a yellow tint, but is not rendered turbid.

The aqueous solution of cinchonetin, which is of a red colour, is not rendered turbid either by ammonia or potash, but they change the colour first to purple and afterwards to fawn colour, and acids do not restore the original tint; subacetate of lead occasions a violet precipitate, which soon subsides; chlorine instantly destroys the colour. By the combined action of sulphuric acid and peroxide of lead, cinchonetin may be converted into fresh colourless compounds, and acetic acid appears to be one of them.

Quina.—The result of the action of nascent oxygen upon this alkali is a complex substance, upon which heat acts as it does upon cinchonetin; and it may be separated into at least two distinct principles, *quinetin* and *modified quinetin.* The former of these remains when the complex colouring matter is treated with water, in which it is insoluble; it dissolves, however, in alcohol, and imparts a violet

colour to it; the modified quinetin, on the contrary, is soluble in water, and the solution has a blood-red colour.

Modified Quinetin is an amorphous mass, of a red colour, has a bitter taste, and is soluble in water, alcohol and æther; when the aqueous solution is boiled, in order to evaporate it to dryness, it is decomposed, and deposits a black powder which is insoluble in alcohol and æther.

Potash and ammonia decolorize the aqueous solution of this substance, which from red becomes of a dirty yellow colour; it is not, however, altered, as cinchonetin is, for by saturating the alkali with sulphuric acid, the original may always be restored. Modified quinetin dissolves in the concentrated sulphuric and nitric acid, and imparts its colour to them.

Quinetin.—By the spontaneous evaporation of the alcoholic solution, this substance may be obtained for the most part crystallized. It is insoluble in water, but dissolves extremely well in alcohol, æther, and water acidulated with sulphuric acid; to the first two solvents it gives a violet colour, and to the last a red one. Although this substance is insoluble in water, its alcoholic solution is not rendered turbid by the addition of water.

Potash and ammonia act upon it in the same way as upon the modified quinetin.

The concentrated acids dissolve quinetin and acquire a red colour; it probably passes to the state of modified quinetin.

By repeated solutions and evaporations, quinetin may be rendered soluble in water, and it then appears to possess all the properties of modified quinetin.

Morphia.—Morphetin amorphous, brown and slightly bitter, little soluble in alcohol, but imparts a yellow colour to it; on the other hand it is very soluble in distilled water, to which it gives a reddish-yellow colour, and the property of reddening litmus paper. Sulphuric and nitric acids turn the colour of this solution to pale yellow, whereas it becomes deeper by the caustic alkalies. Subacetate of lead does not alter or precipitate this solution; concentrated sulphuric acid dissolves morphetin with difficulty, but nitric acid dissolves it readily and acquires a yellow colour. Morphetin, when submitted to the action of peroxide of lead, till effervescence ceases, is converted into a yellow deliquescent uncrystallizable substance, possessing acid properties.

Narcotina.—Narcotein is an amorphous, uncrystallizable mass of a brown colour and very bitter taste; it is very slightly soluble in æther, but readily so in water and alcohol; it is soluble in concentrated nitric acid, and renders it yellow; it also dissolves, and with great facility, in concentrated sulphuric acid, rendering it of a magnificent red colour, which becomes yellow on the addition of water.

The aqueous solution of narcotein is of a yellow colour; when either ammonia or potash is added to it, the colour becomes rather brighter, and reddish-brown. Subacetate of lead does not alter cither its colour or transparency.

When narcotein is subjected to the action of nascent oxygen, until

effervescence ceases, it is slightly decolorized, and is converted into a new substance, the opianic acid, previously obtained by MM. Liebig and Wœhler in their experiments on the oxidizement of narcotina.

Strychnia.—This treated like the preceding yields a pulverulent mass of a brownish yellow colour, very slightly soluble in boiling water, but imparts to it a dirty yellow colour and a bitter taste; it is very slightly soluble in alcohol, but soluble in æther, and also in boiling water acidulated with sulphuric acid; and from this the greater part precipitates on cooling.

Potash is the best solvent of this substance; the solution has a brown colour, and if only the exact quantity of potash required for solution be employed, the solution does not act on syrup of violets. When sulphuric acid is added to the solution, so as to give it an acid reaction, it becomes turbid, and deposits a yellowish flocky precipitate. The neutralized alkaline solution, when perfectly neutralized, does not act upon cupreous or ferruginous salts, but it precipitates the salts of lead and silver.

This substance is worthy of the attention of chemists; the remarkable manner in which it acts with potash, appearing to saturate it perfectly, will perhaps lead to its being considered as a new acid.

Brucia.—By the action of nascent oxygen on this alkali two colouring matters are obtained; one which is soluble in boiling alcohol, to which it gives a yellowish-red colour; this will be described as A; the other, B, is insoluble in boiling alcohol, but when diluted with an equal volume of water it is then dissolved readily; the resulting solution has a fine red colour.

A. This is a brown, uncrystallizable substance; it has a very bitter taste, and is very little soluble in æther, but is dissolved by boiling alcohol; distilled water dissolves it and acquires a yellowish-red colour; it is soluble in concentrated nitric acid, and produces as fine a colour as brucia; sulphuric acid also dissolves it, but more slowly; and the solution has also a very fine red colour; hydrochloric and acetic acids and potash also dissolve it very well, but the solutions have a yellow colour.

B. This substance, like the preceding, is uncrystallizable; its taste is bitter, and when in mass it is black, but in thin portions its colour is red; it is insoluble in æther and in boiling alcohol, but dissolves very well in the latter when diluted with water; it is soluble in distilled water, to which it imparts a fine wine-red colour, which acids brighten slightly, whilst potash renders it brownish and subacetate of lead yellow.

The concentrated sulphuric and nitric acids dissolve this substance, and so does potash, the solutions acquiring a yellowish-red colour of less or greater intensity.—*Journ. de Chim. Médicale*, Juillet 1844.

ON THE HYDRURET OF COPPER. BY M. AD. WURTZ.

The author remarks that the only well-authenticated metallic hydrurets hitherto described are those of arsenic, antimony and potassium.

On examining the action of hypophosphorous acid on the salts of copper, the author found, that under certain circumstances a compound was formed which possessed all the characters of a definite hydruret of copper; it may be prepared in the following manner :—

Dissolve one part of hypophosphite of barytes in water, and precipitate the barytes, perfectly, by sulphuric acid; add to the filtered liquor 0·8 of a part of sulphate of copper in a concentrated solution. The mixture is to be heated to a temperature not exceeding 158° Fahr.; the liquor assumes a green tint, and then a precipitate is formed, which is at first yellow, but gradually deepens till it becomes of the colour of kermes; at this point of the operation small bubbles of hydrogen are often observed to be given out; in this case the liquor must be quickly cooled, and the precipitate is to be washed in an atmosphere of carbonic acid, with water free from air; it then only remains to dry the substance by pressure between folds of filtering paper.

With respect to the theory of this operation, the author remarks, that the first effect of heat is to reduce the sulphate of copper to the sulphate of suboxide by the agency of the hypophosphorous acid, the salt remaining in solution. This formation of a salt of suboxide of copper may be proved by the addition of a few drops of sulphurous acid, which in a short time precipitates red-brown sulphate of suboxide. If the heat be continued, the salt of copper is also reduced, and a hydruret of copper is formed, which corresponds in composition to the suboxide of copper.

This reaction is never quite perfect; even when there is an excess of hypophosphorous acid, there always remains some oxide of copper in solution when operating in the mode described; but if the proportions of the mixture, the nature of the salts of copper and the conditions of temperature be changed, the products of the reaction are different, and these the author proposes to describe in a future memoir on the hypophosphites.

Hydruret of copper is a powder of a deep brown colour; when dry it begins to decompose, though slowly, at 131° Fahr. If it be heated in an open vessel to 140°, the decomposition is often sudden, and occurs with a kind of whistling throughout the mass, which is quickly projected. In the moist state the hydruret of copper is rather more stable; *in vacuo* it loses hydrogen; when exposed to the air it is gradually converted into a yellow powder, which is suboxide of copper; this oxidation occurs more rapidly if the hydruret be moistened. In a perfectly dry air this substance blackens in a day's time, probably because a little oxide of copper is formed.

Dry hydruret of copper inflames in chlorine with the formation of thick vapours, which condense into flocculi of perchloride of copper; similar incandescence occurs in bromine; hydrochloric acid exerts a very remarkable action on hydruret of copper; the concentrated acid, even cold, produces a brisk effervescence of hydrogen, and protochloride of copper is formed. If too great excess of acid be not employed, a portion of this salt crystallizes in small scales, which are readily distinguishable among the small residue of copper. On the

addition of a little water, the liquor becomes milky, and it presents in other respects all the characters of the proto-salts of copper. From these statements it is evident that the hydruret of copper and the hydrochloric acid are both decomposed. This fact was ve-rified by direct experiment; when a certain portion was decomposed by heat alone, and an equal portion by hydrochloric acid, double the quantity of hydrogen gas was obtained in the latter case. It is well known that hydrochloric acid acts upon copper with great dif-ficulty, and the presence of hydrogen, so far from favouring its action, offers a fresh obstacle to it; the decomposition, therefore, of hydru-ret of copper by hydrochloric acid appears to be the result of the action of contact.

The results of four analyses of this compound were as follows:—

Hydrogen ..	1·220	1·215	1·221	1·229
Copper	98·780	98·785	98·779	98·771
	100·	100·	100·	100·

M. Wurtz, considering that there is an unavoidable loss of hydro-gen, is disposed to regard this hydruret as composed of 1 equiva-lent of hydrogen and 2 equivalents of copper, which, taking hydro-gen = 1 and copper = 32, would give in 100 parts, very nearly,

Hydrogen .... 1·54
Copper........ 98·46
100·

Ann. de Ch. et de Phys., Juin 1844.

ON THE PREPARATION AND PROPERTIES OF CERTAIN CHLO-RATES. BY M. ALEXANDER WAECHTER.

Chlorate of Strontia.—Carbonate of strontia was dissolved in chloric acid; the solution evaporated over sulphuric acid to the con-sistence of a syrup, and yielded large pyramidal crystals, which could not be measured on account of their becoming quickly moist in the air; they are insoluble in alcohol; when heated they split without losing water, and fuse at about the same temperature as the barytic salt. If they be more strongly heated, they yield oxygen with a trace of chlorine, and there remains chloride of strontium, which is alkaline. When mixed with combustible bodies and struck, this salt detonates like chlorate of potash. This salt is composed of

Chloric acid .. 59·29
Strontia...... 40·71
100·

Chlorate of Lime.—A neutral solution of carbonate of lime in chloric acid, gave by evaporation over sulphuric acid, to the consist-ence of a syrup, well-defined crystals of chlorate of lime; they are oblique rhombic prisms, which could not be measured on account of their deliquescing in the air; they are very soluble in alcohol and give its flame a fine red colour; if they be quickly heated to a little below 212° Fahr., they fuse in their water of crystallization; if, on

the contrary, the temperature be slowly raised, they lose 14·1 per cent. of water without fusing; they are then anhydrous, and fuse only at a temperature a little lower than the salts of barytes and strontia.

Like these salts also the chlorate of lime loses oxygen at a high temperature, and chloride of calcium with an alkaline reaction remains. This salt consists of

Chloric acid ..	62·54
Lime	23·36
Water	14·10
	100·

Chlorate of Magnesia.—The neutral solution of this salt, obtained by decomposing chlorate of barytes by sulphate of magnesia, yielded by evaporation over sulphuric acid, a saline mass of crystalline laminæ, which was very deliquescent and readily soluble in alcohol. It was separated from the solution, pressed between folds of blotting-paper, and then perfectly dried over sulphuric acid; it fused at 120° Fahr.; and at about 248° Fahr. it begins to lose water, oxygen and chlorine. When the heat is continued there is obtained a mixture of magnesia and chloride of magnesium; if it be heated very rapidly magnesia only is left.

It is composed of

Chloric acid ..	50·25
Magnesia	13·77
Water	35·98
	100·

Protochlorate of Manganese.—This compound could not be obtained in the solid form. The colourless solution obtained by decomposing chlorate of barytes with protosulphate of manganese, decomposes by evaporation over sulphuric acid, at a certain degree of concentration, into chlorine, oxygen and hydrated peroxide of manganese; the author therefore merely states the properties of a dilute solution: it suffers no change by ebullition; sulphuric acid diluted gives it a deep red colour; there being formed sulphate of manganese at the expense of the oxygen of the chloric acid. If a small quantity of solution of protoxide of manganese be added to an excess of concentrated sulphuric acid, a brown crystalline precipitate is formed, which is an intimate mixture of peroxide of manganese and sulphate of the protoxide, insoluble in concentrated sulphuric acid.—*Journ. de Pharm. et de Ch.*, Mai 1844.

PREPARATION OF PROTOXIDE OF GOLD. BY M. L. FIGUIER.

The author states that the substance which Berzelius describes as protoxide of gold is a mixture of the oxide and metallic gold; and he mentions the following processes for procuring the real protoxide:—

1st. Let a solution of chloride of gold be evaporated to dryness to deprive it of acidity; to the residue dissolved in water add a solution

of protonitrate of mercury, and a deep violet precipitate of protoxide of gold is formed; sometimes the precipitate does not separate, and the oxide remains apparently dissolved; but by heat, the precipitate separates immediately; in this operation it is essential not to use an excess of the mercurial salt, for it would produce protochloride, on account of the presence of the hydrochloric acid which accompanies the reaction; it is sufficient not to precipitate the whole of the gold, and to stop while the solution remains slightly coloured yellow with the undecomposed chloride of gold.

2nd. The decomposition of protochloride of gold by potash is a good method of preparing protoxide of gold, but there are precautions which are indispensable to be observed in order to obtain a pure product: evaporate a solution of chloride of gold to dryness, and heat the residue on a sand-bath, the temperature of which is determined by a thermometer; the mass is to be continually agitated till it assumes a very bright canary-yellow colour, taking care that the temperature does not exceed 302° Fahr.; at a higher temperature the protochloride loses the greater part of its chlorine.

If potash be then poured on the mixture, a dark violet precipitate of protoxide of gold separates, and the solution has a deep yellow colour, owing to the potash having dissolved a part of the protoxide of gold; after filtration nitric acid is to be added, so as very accurately to saturate the potash, and precipitate the oxide of gold which it held in solution, in the state of a gelatinous hydrate of a deep violet colour; excess of nitric acid must be avoided, for the *aqua regia* which it would form, would dissolve the product; it is better to let the solution remain slightly alkaline; the products of these operations are to be collected on the same filter.

3rd. If concentrated acetic acid be boiled with tritoxide of gold obtained by decomposing aurate of potash with nitric acid, the filtered liquor has a deep yellow colour, and by evaporating it nearly to dryness, the separation of a very considerable quantity of protoxide of gold takes place.

4th. The tritoxide of gold (auric acid) recently prepared from the aurate of magnesia or barytes, being diffused through a solution of potash or soda, readily dissolves and forms a solution of a golden yellow colour; if this solution be submitted to evaporation, protoxide of gold is always precipitated, whatsoever means of evaporation be employed, either without the contact of air or spontaneous evaporation.

This precipitation is especially active at a boiling heat; the solution becomes quickly turbid and deposits a greenish precipitate, probably the tritoxide, and this is soon followed by an abundant deposit of violet protoxide of gold; this reaction continues in a singular manner, and solutions which contained only about 300 grains of gold, will continue to become turbid and to deposit protoxide of gold, after having boiled for nearly two days.

5th. The neutral chloride of gold, treated with potash or soda, forms, after long-continued ebullition, so considerable a quantity of

protoxide that this method may also be used. The carbonate and bicarbonate of potash produce the same effects.

6th. Vegetable infusions, treated with chloride of gold and excess of a fixed alkali, produce, at a moderately high temperature, or still better by contact of some hours' duration, an abundant precipitate of protoxide of gold, retaining, however, a little organic matter.

7th. The acetate, citrate and the tartrate of potash, with an excess of potash, form under the same circumstances protoxide of gold.

The preceding facts seem to determine pretty clearly the action of organic salts upon chloride of gold: when an alkali is present, protoxide is precipitated; but if the salts be neutral or acid, metallic gold is thrown down: these facts are proved by a very simple experiment; when solution of gold is boiled, tartaric acid or neutral tartrate of potash, a precipitate of gold is formed; but if the liquor be filtered which contains some undecomposed chloride of gold, and potash be added in excess, a blackish-violet precipitate of protoxide is immediately obtained.—*Ann. de Ch. et de Ph.*, Juillet 1844.

[The properties and composition of the protoxide and some other compounds of gold will be given in a future number.—ED. P. M.]

METEOROLOGICAL OBSERVATIONS FOR JUNE 1844.

Chiswick.—June 1. Clear and fine. 2. Overcast and cold: fine: cloudy. 3. Light clouds and very fine. 4, 5. Very fine. 6. Slight rain: cloudy. 7. Overcast: boisterous. 8. Very fine. 9. Slight rain: very fine. 10. Fine: cloudy. 11—16. Very fine. 17. Hot and dry: cloudy. 18. Rain: fine. 19. Overcast: heavy clouds, with showers. 20. Overcast. 21, 22. Very fine. 23. Exceedingly clear: sultry. 24. Cloudy: hot and sultry. 25. Constant heavy rain. 26. Cloudy: fine. 27. Cloudy. 28, 29. Very fine. 30. Dry haze: overcast and fine.—Mean temperature of the month 2°·19 above the average.

Boston.—June 1. Fine. 2, 3. Cloudy. 4. Fine. 5. Cloudy. 6. Rain early A.M.: rain A.M. 7. Cloudy. 8. Cloudy: thermometer at 4 o'clock 75°. 9. Cloudy. 10. Fine: rain A.M. 11. Fine. 12. Fine: thermometer 4 o'clock 75°. 13. Fine: stormy all day. 14, 15. Stormy. 16, 17. Fine. 18. Cloudy. 19, 20. Cloudy: rain A.M. and P.M. 21. Cloudy. 22. Fine. 23. Fine: thermometer at noon 81°. 24. Fine: rain early A.M., with thunder and lightning: thermometer at noon 80°. 25. Cloudy: rain A.M. and P.M. 26, 27. Cloudy. 28—30. Fine.

Sandwick Manse, Orkney.—June 1. Cloudy. 2, 3. Bright: cloudy. 4. Showers: cloudy. 5—7. Showers: rain. 8. Bright: cloudy. 9. Cloudy: showers. 10, 11. Bright: drops. 12. Bright: rain. 13. Showers: bright. 14, 15. Showers. 16. Bright: clear. 17. Clear. 18. Drizzle. 19. Drizzle: drops. 20. Showers: drops. 21. Clear. 22. Fog. 23. Drops: clear. 24. Hazy: clear. 25. Clear: cloudy. 26, 27. Cloudy. 28. Cloudy: damp. 29, 30. Cloudy.

Applegarth Manse, Dumfries-shire.—June 1. Dry and withering. 2. Dry and withering: cloudy. 3. Fine. 4. Cloudy and threatening rain. 5, 6. Rain. 7. Very wet. 8. Fair, but cloudy. 9. Fair: threatening. 10. Showers. 11. One slight shower. 12, 13. Heavy rain. 14, 15. Fair. 16. Fair and fine. 17, 18. Rain. 19. Fair. 20, 21. Rain. 22. Fair. 23. Fair and warm: thunder. 24. Rain. 25. Showery. 26—30. Fair and fine.

Mean temperature of the month	55°·1
Mean temperature of June 1843	54 ·7
Mean temperature of spring-water	51 ·6
Mean temperature of ditto June 1843	50 ·7

Meteorological Observations made by Mr. Thompson at the Garden of the Horticultural Society at Chiswick, near London; by Mr. Veall, at Boston; by the Rev. W. Dunbar, at Applegarth Manse, Dumfries-shire; and by the Rev. C. Clouston, at Sandwick Manse, Orkney.

Days of Month. 1844. June.	Barometer. Chiswick Max.	Chiswick Min.	Boston 8½ a.m.	Dumfries-shire 9 a.m.	Dumfries-shire 9 p.m.	Orkney, Sandwick 9¼ a.m.	Orkney, Sandwick 8½ p.m.	Thermometer. Chiswick Max.	Chiswick Min.	Boston 8¾ a.m.	Dumfries-shire Max.	Dumfries-shire Min.	Orkney, Sandwick 9¼ a.m.	Orkney, Sandwick 8½ p.d.	Wind. Chiswick 1 p.m.	Boston	Dumfries-shire	Orkney, Sandwick	Rain. Chiswick	Boston	Dumfries-shire	Orkney, Sandwick
1.	29·998	29·949	29·55	30·06	29·97	30·11	30·13	72	44	57	67	44	56	45	ne.	calm	ene.	w.				
2.	29·971	29·953	29·50	29·98	29·96	30·14	30·14	61	41	52·5	58½	46½	48	40	ne.	calm	e.	nw.				
3.	30·123	30·057	29·55	30·00	30·00	30·14	30·04	69	41	53	64½	41	50	48	e.	calm	se.	w.				
4.	30·147	30·057	29·60	29·94	29·86	29·92	29·93	79	40	60	64	52	50	46	sw.	calm	sw.se.	w.				·20
5.	29·976	29·882	29·39	29·69	29·60	29·80	29·63	79	53	64	58½	52	49	48	sw.	calm	se.sw.	se.	·06			
6.	29·834	29·714	29·05	29·50	29·55	29·69	29·60	70	53	58	64	53	55	48	sw.	sw.	se.sw.	s.	·02	·08		·16
7.	29·901	29·862	29·22	29·56	29·40	29·57	29·60	71	50	63	60½	52	56	54	sw.	sw.	s.	sw.		·11	1·02	·22
☾ 8.	30·057	30·001	29·32	29·78	29·81	29·72	29·78	80	49	66	65	52	50	53	sw.	w.	s.	sw.	·02			·30
9.	30·009	29·983	29·29	29·78	29·80	29·74	29·73	80	51	63·5	61	52	62	53½	w.	calm	sw.	wsw.	·01			
10.	30·110	30·034	29·43	29·77	29·90	29·71	29·78	77	41	63·5	60	47	60	53	w.	w.	wsw.	sw.				·24
11.	30·186	30·143	29·63	30·00	30·00	29·92	29·97	83	42	65	63	45	52½	53	sw.	w.	wsw.	wsw.		·16		·08
12.	30·166	30·080	29·55	29·96	29·78	29·90	29·62	82	56	66	63½	50	53½	52	sw.	w.	sw.	sw.				·18
13.	30·003	29·940	29·28	29·56	29·63	29·45	29·30	85	52	70	66	53	58	50	sw.	w.	wsw.	w.				·22
14.	30·046	29·999	29·35	29·68	29·74	29·93	29·50	75	49	63·5	58½	48½	47	49	w.	nw.	wsw.	nw.			0·42	·39
15.	30·046	30·009	29·42	29·80	29·92	29·71	29·92	78	43	58	57	47	47	48½	w.	calm	wnw.	nw.				·18
● 16.	30·197	30·168	29·64	30·05	30·08	30·04	30·11	78	40	62	65	42	50	46	nw.	calm	wnw.	w.				
17.	30·153	29·925	29·67	30·03	29·83	30·08	30·03	77	52	64	57½	45½	55	48	s.	calm	sw.	ne.	·04			
18.	29·757	29·676	29·28	29·72	29·70	30·00	30·00	77	54	57	58½	46½	49½	50	e.	calm	n.	n.	·05			
19.	30·014	29·858	29·35	29·84	29·86	29·98	29·92	66	46	59	52	40½	45	47	nw.	calm	nw.	w.	·02			
20.	30·041	29·972	29·54	29·80	29·70	29·79	29·75	74	53	56	58½	53½	49½	46	w.	calm	s.	w.	·01			·10
21.	29·980	29·761	29·42	29·67	29·60	29·71	29·70	83	52	63·5	63	54	51	48	sw.	calm	sw.	ese.				·05
22.	29·797	29·769	29·19	29·55	29·68	29·63	29·73	86	48	66	64½	54	55	49	sw.	calm	s.	e.		·08		
☽ 23.	29·789	29·742	29·25	29·70	29·69	29·78	29·82	91	63	73	71½	58	55	53	s.	calm	s.	ese.		·09	1·46	
24.	29·651	29·642	29·04	29·61	29·62	29·88	29·90	89	67	73	69	53	52	55	sw.	calm	ne.	ne.		·13		
25.	29·666	29·613	29·00	29·66	29·69	29·95	29·91	65	47	62	59	46	52	50	sw.	calm	ne.	ne.	·62	·05		
26.	29·784	29·718	29·26	29·80	29·77	29·92	29·92	67	51	59	55½	47	48½	47½	n.	calm	ne.	n.	·12	·79		
27.	29·886	29·828	29·39	29·85	29·80	29·92	29·90	68	50	55·5	60	46	47½	47	w.	calm	s.	nw.				
28.	30·016	29·949	29·50	29·97	29·90	29·94	30·00	74	47	58·5	67½	48	51	48½	sw.	calm	s.	nw.				
29.	30·065	29·926	29·57	29·70	29·94	30·02	30·04	83	54	63	67	47	52	48	ne.	w.	ssw.	nw.			0·53	
○ 30.	29·936	29·850	29·47	29·90	29·79	30·00	29·96	79	50	65	67		50	47		calm	se.					
Mean.	29·976	29·902	29·39	29·797	29·445	29·865	29·841	76·60	49·30	61·9	62·4	48·¾	51·93	49·10					0·97	1·49	3·43	2·32

XXIV. *On Coal-Gas. By* Thomas Thomson, *M.D., F.R.S. L. and E., M.R.I.A., Regius Professor of Chemistry in the University of Glasgow*[*].

IT is well known that the word *gas* was first introduced into chemistry by Van Helmont in his *Treatise de Flatibus.* Junker, whose *Conspectus Chemiæ Theoretico Practicæ* was published in 1744, conjectures that Van Helmont's word gas was merely the German word *gäscht,* fermentation, in a Latin dress, and this conjecture seems as probable as any.

Boyle was the first chemist who attempted to make gas artificially, and who showed that thus prepared it possessed the mechanical properties of common air. The gas which he examined was hydrogen, obtained by pouring dilute sulphuric acid on iron filings.

Hales, in 1726, proved by experiment that many animal and vegetable substances, when heated sufficiently, give out an air which possesses the mechanical properties of common air, and which therefore he considered as not differing in its properties from common air. That hydrogen gas was combustible was known at least as early as the beginning of the last century, and many remarkable stories are told by early chemists of the eighteenth century, about its combustibility, and the violent explosions which a mixture of it and common air produced when brought in contact with a burning body.

Dr. Black first showed that carbonic acid, though a gas, differed essentially from common air, and he gave it the name of *fixed air,* because it existed in a solid state in the carbonates. Cavendish, in 1766, showed that hydrogen differs from common air and from carbonic acid; he examined its combustibility, its specific gravity, and pointed out its peculiarities. In 1772 Dr. Priestley began his experiments on air; first he exa-

* Read before the Philosophical Society of Glasgow, November 1, 1843; and communicated to this Journal by the author.

mined carbonic acid and hydrogen, then azotic gas, then deut-
oxide of azote, muriatic acid gas and ammoniacal gas. In
1774 he discovered sulphurous acid gas, and oxygen gas, which
was destined to make such an alteration in the chemical theo-
ries of the time. He discovered fluoric acid gas and carbonic
oxide, though he was not aware of its peculiar nature, and
indeed remained ignorant of it to the end of his life.

It is curious that Dr. Priestley nowhere, so far as I know,
mentions carburetted hydrogen or heavy inflammable air, as
it was then called. It constitutes the *fire damp* of coal mines;
its combustibility and its property of exploding with great vio-
lence in certain circumstances must have been known in coal
countries at a pretty early period. In the Philosophical
Transactions for 1667, there is an account of a blower of this
gas passing through and taking fire from the flame of a candle,
and burning briskly; and in the same work there are many
histories of explosions in coal mines attended with the loss of
many lives.

Though carburetted hydrogen occurs so commonly in coal
mines, and though it burns with a strong flame and gives out
a good deal of light, and although it had been ascertained that
when common coal was distilled at a red heat it gave out a
great deal of inflammable gas, it does not seem to have oc-
curred to any person to employ it as a substitute for candles,
till the idea struck Mr. Murdoch, an Ayrshire gentleman in
the employ of Watt and Boulton. In the year 1808 he pub-
lished a paper in the Philosophical Transactions, pointing out
the advantages that would result from employing coal-gas in-
stead of oil for illuminating the streets of towns and manu-
factories *.

In this paper he gives an account of the apparatus which
he had fitted up for lighting the cotton manufactory of Messrs.
Phillips and Lee at Manchester, which was at that time the
greatest cotton mill in the kingdom. He shows that the ex-
pense was only about one-fourth of that of the candles or oil ne-
cessary to produce the same quantity of light that the gas did.
The coal used was the best Wigan cannel, a ton of which he
says yields 7160 cubic feet of gas, and produces about two-
thirds of a ton of coke.

In this interesting paper Mr. Murdoch gives the history of
the discovery of gas making. In the year 1792, while at Red-
ruth in Cornwall, he made a set of experiments on the quan-
tity and qualities of the gases produced by distillation from
different mineral and vegetable substances. He was induced
by some observations which he had previously made on the

* [Mr. Murdoch's paper was reprinted in Phil. Mag. S. 1. vol. xxxii.,
p. 113.—EDIT.]

burning of coal to try the combustible properties of the gases produced from it, as well as from peat, wood and other in- flammable substances; and being struck with the great quan- tities of gas which they afforded, as well as with the brilliancy of the light and the facility of its production, he instituted se- veral experiments with a view of ascertaining the cost at which it might be obtained, compared with that of an equal quantity of light yielded by oils or tallow.

In the year 1798 he removed from Cornwall to Boulton and Watt's works at Soho, and there he constructed an appa- ratus upon a larger scale, which during many successive nights was applied to the lighting of their principal building; and various new modes were tried for washing and purifying the gas. These experiments were continued with some inter- ruptions till the peace of 1802, when a public display of the gas-light was made by him in the illumination of the manu- factory at Soho on that occasion.

Since that period, or between it and 1808, he extended the apparatus at Soho, so as to give light to all the principal shops, where it was in regular use to the exclusion of other arti- ficial light. In 1808 he fitted up the gas apparatus in Messrs. Phillips and Lee's cotton mill, since which time it has been extended to all the cotton mills in the kingdom.

I have stated these details, though but imperfectly connected with the subject which I mean to discuss, because I believe the history of the introduction of gas as a substitute for oil or candles is not very generally known. It is commonly ascribed to Mr. Windsor, who took out a patent in 1806 and who de- livered lectures on the subject several years after, and who endeavoured to get up a joint stock company, with what suc- cess I do not know. Several attempts were made here about the year 1808, and during the winter of that year the front of the Tontine buildings at the Cross of Glasgow was lighted with gas for several weeks. London was the first city illumi- nated with gas. Philip Taylor erected the gas-works at Paris soon after the peace of 1815.

In the preceding historical sketch I have taken no notice of Lord Dundonald's coal-tar works at Culross; the current of gas escaping from his ovens was frequently fired; but it does not seem to have occurred to him to employ the gas thus extricated for œconomical purposes. Nor have I noticed M. Lebon, who is said in 1786 to have attempted, but without success, to employ gas distilled from wood as a substitute for candles; these attempts led to no results and were speedily forgotten.

There are four varieties of coal which have been tried in Great Britain in the manufacture of gas, namely, caking coal,

cherry coal, splint coal and cannel coal. Of these the cannel coal, or parrot coal as it is called here, yields the best gas; the caking coal, or Newcastle coal, yields the worst, and the cherry and splint, though very different in their appearance, yield an intermediate gas, the quality of which, whether from cherry or splint coal, is nearly the same.

There are three varieties of cannel coal in the neighbourhood of Glasgow, named from the localities where they occur, Skaterigg, Lesmahagow and Monkland.

The specific gravity of these varieties of coal is as follows:—

  Caking coal . . 1·280 Mr. Richardson.
  Cherry coal . . 1·268   ...
  Splint coal . . . 1·307   ...
  Skaterigg . . . 1·229 Dr. R. D. Thomson.
  Lesmahagow . 1·198   ...
  Monkland . . . 1·189   ...

Besides ashes, these six varieties of coal consist of carbon, hydrogen, azote and oxygen, combined in various proportions according to the coal. I shall here give the composition of each; that of the first three was determined by Mr. Richardson of Newcastle in the laboratory at Giessen; that of the last three in the College laboratory by Dr. R. D. Thomson. The azote is small in quantity, so small that Mr. Richardson did not succeed in determining its exact quantity; but we found no difficulty in coming to very exact conclusions by the process of Will and Varrentrapp*. As the quantity in all our varieties tried varied from 1·48 to 1·75 per cent., I have supposed that the azote in the three varieties determined by Mr. Richardson was the mean of these two quantities, or 1·61 per cent. The following table shows the composition of these coals:

	Mr. Richardson.			Dr. R. D. Thomson.		
	Caking coal.	Cherry coal.	Splint coal.	Skaterigg coal.	Lesmahagow coal.	Monkland coal.
Carbon ...	87·952	83·025	82·924	76·20	76·25	70·02
Hydrogen.	5·239	5·250	5·491	5·44	6·07	5·56
Azote......	1·610	1·610	1·610	1·75	1·61	1·48
Oxygen ...	3·806	8·566	8·847	14·47	12·26	14·86
Ashes......	1·393	1·549	1·128	2·14	2·81	8·08
	100·	100·	100·	100·	100·	100·

It will facilitate our conception of the composition of these different coals if we exhibit their condition by empirical formulas representing the atoms of each constituent, the quantity of azote being reckoned one atom: we leave out the ashes, because they have nothing to do with the production of the gas, excepting that they materially influence its quantity.

* This process was first proposed by Dr. Schafhaeutl, in Phil. Mag. [S. 3. vol. xvi. p. 44.]

$$\text{Caking} \ldots \ldots C_{127} H_{53} Az O_4$$
$$\text{Cherry} \ldots \ldots C_{121} H_{46} Az O_9$$
$$\text{Splint} \ldots \ldots C_{120} H_{45} Az O_{10}$$
$$\text{Skaterigg} \ldots C_{102} H_{43} Az O_{15}$$
$$\text{Lesmahagow} \ldots C_{110} H_{52} Az O_{14}$$
$$\text{Monkland} \ldots C_{111} H_{53} Az O_{18}$$

It appears from this table that Newcastle coal contains the most carbon, and Monkland cannel coal the least; while cannel coal contains the most oxygen and Newcastle coal the least; Newcastle coal contains the least hydrogen and cannel coal the most. Now cannel coal yields the best and Newcastle coal the worst gas. This need excite no surprise; carbon not being volatile, it is obvious that if coal contained nothing but carbon it would yield no gas at all. Coal-gas is a mixture of four different gases, most of which are compounds; two are compounds of carbon and hydrogen, one of carbon and oxygen, and the fourth is pure hydrogen. There is no difficulty in conceiving the formation of the gaseous compounds of carbon and hydrogen, but it is not so easy to explain the occurrence of carbonic oxide and hydrogen. These two gases are never entirely wanting; at least I have analysed above forty specimens of coal-gas from different kinds of coal and from different gas-works without ever failing to find them. I think it probable that they make their appearance towards the end of the process of heating the coal. It is well known that the longer the process of gas making is continued, and the higher the temperature at which the gas is produced, the worse is the gas, and of course the more hydrogen it contains. Is it not possible that coal may contain water; that this water can only be extricated at a high temperature; that its oxygen combines with carbon and forms carbonic oxide, while the hydrogen makes its escape in the gaseous state? If this supposition were true, there ought to be a constant ratio between the volume of carbonic oxide and hydrogen in the coal-gas; but this not being the case, it is obvious that the supposition cannot be well founded*.

* Mr. John Hart, of this city, states that he made an experiment which appears to explain the appearance of the hydrogen towards the end of the process. "Having conceived the idea many years ago of causing gas to take up an additional dose of carbon, by passing it over red-hot charcoal, he procured a 1½-inch iron pipe, and having charged it with charcoal he passed it through the furnace below the gas retort, and joined one end with the pipe from the retort and the other to the pipe leading to the condenser; the fire was then applied and the retort charged as usual. After the gas-holder had risen about a foot, he observed the lead pipe leading to the condenser becoming very hot; it soon after gave way and fell to pieces, and the whole of the gas escaped into the air; but it had no longer the yellow silky appearance of gas issuing from a retort; it had become a white vapour, and had also lost the smell. When the charcoal was examined it

A ton of Lesmahagow coal, when distilled at the usual temperature, yields about (10,080 cubic feet) one-fifth of its weight of gas, two-fifths of coke, and two-fifths of tar, water, &c.

The gas contains about one-fifth of the carbon in the raw coal, two-elevenths of the hydrogen, and two-ninths of the oxygen; about one-half of the carbon remains in the state of coke, so that about two-fifths go to the formation of the naphthalin, naphtha, naphthene, naphthol, &c., which are formed during the distillation.

Nine-elevenths of the hydrogen and seven-ninths of the oxygen go to the formation of water and various other compounds. The ammonia formed amounts to about 1 per cent. of the liquor obtained during the distillation of the coal.

When gas-works were first established the coal was distilled in iron retorts, but it has been found more œconomical to substitute vessels of stoneware, or rather indeed ovens of firebrick made air-tight; these I believe are generally superseding the iron retorts.

During the course of last winter I made thirty-five analyses of gas from different gas-works, but most commonly Glasgow gas. The gas which I used was taken from a pipe at some distance from the gas-works, because the gas required to be washed and purified before it was examined. After turning the stopcock the gas was kindled and allowed to burn for several minutes before I began to collect it. In every case it contained a mixture of common air, which varied in different specimens of gas from 4 per cent. to 28 per cent.; the mean quantity was $12\frac{1}{2}$ per cent. The specimen containing 28 per cent. of common air was brought up from Greenock, and though very great care was taken in packing it, it is possible that at least a portion of this air might have made its way into the bottles during the transit. If we omit this specimen, the average quantity of common air in the Greenock gas was $10\frac{1}{2}$ per cent; the average quantity in the Glasgow gas was $12\frac{1}{3}$ per cent.

I think it most likely that the common air, which forms a constant ingredient in all gas from gas-works that I have examined, had made its way into the pipes, which it must be very difficult to make air-tight; and when the pressure is removed common air will undoubtedly enter wherever it can find access. The Greenock gas was collected in an apartment very near the gas-works; the Glasgow gas was collected

was found to be covered over with a fine smooth, shining black coat of carbon, which had been deposited on it." Mr. Hart has concluded from this and other facts, that the gas is decomposed by the red-hot coke, and proposes that the coal should be applied in thin layers to the interior surface of the retorts.

in my laboratory, which may be about a furlong from the gas-works; now the average quantity in the Greenock gas was $10\frac{1}{2}$, and in the Glasgow $12\frac{1}{3}$.

The highest specific gravity of the Glasgow gas was 0·582, and the lowest 0·463; the average was 0·502.

The quantity of olefiant gas in Glasgow gas varied from 11·77 per cent. to 17·83 per cent; the mean quantity was 13·52 per cent.

I got gas made at Greenock with as much care as possible from each of the three varieties of cannel coal found in the neighbourhood of Glasgow, namely Skaterigg, Lesmahagow and Monkland: the specific gravities of these gases were,

Skaterigg . . 0·497
Lesmahagow 0·560
Monkland . . 0·622

The olefiant gas per cent. contained in each was as follows:—

Glasgow . . . 13·52
Skaterigg . . 14·15
Lesmahagow. 16·66
Monkland . . 22·15

Mr. Ritchie, the manager of the Greenock gas-works, who prepared these gases, told me that he thought rather too much heat had been applied to the Lesmahagow coal, which in his opinion would have somewhat deteriorated the Lesmahagow coal-gas.

The mean quantity of carburetted hydrogen gas in the Glasgow coal-gas was 60·6 per cent; the smallest quantity was 47·33, and the largest 79·77: the quantity of this gas in the gases from cannel coal was as follows:—

Skaterigg . . 66·49
Lesmahagow. 59·94
Monkland . . 48·77

The goodness of these gases is in the order of naming them. It would appear from this, that the smaller the proportion of carburetted hydrogen the better is the gas; the reason is that the olefiant gas increases as the carburetted hydrogen diminishes.

The average quantity of carbonic oxide in Glasgow gas was 12 per cent., the smallest quantity was 6·34 per cent., and the greatest quantity 15 per cent.: the quantity of this gas in the three gases from cannel coal was as follows:—

Skaterigg . . 7·07
Lesmahagow. 12·00
Monkland . . 11·76

The mean quantity of hydrogen gas in Glasgow gas was 12·44 per cent., the greatest quantity was 22·85 per cent., and the

smallest quantity 2·21 per cent: the quantity in the three gases from cannel coal was as follows:—

<div style="text-align:center">

Skaterigg . . 12·29

Lesmahagow. 11·46

Monkland . . 17·32

</div>

The common method of determining the light emitted by gas during its combustion, is to set fire to a jet of a given height and issuing from an orifice of a given diameter, and to compare it with the light given out by a wax candle of six in the pound, usually denominated short sixes; an opake body is placed on a sheet of paper, horizontally between the two flames, and it is so placed that the two shadows formed by it are of equal intensity. The distance between this opake body and the flames is measured, and the light emitted by each is as the square of that distance; thus, if the distance between the gas flame and the opake body be two feet, while its distance from the flame of the candle is only one foot, then the light given out by the gas is four times as great as that of the candle.

The light given by the combustion of a jet of Glasgow gas, issuing from an orifice of $\frac{1}{30}$th of an inch in diameter and four inches in height, was as follows:—

1. On the north side of the river $=2\cdot68$ candles.

2. On the south side of the river $1\cdot77$ ...

This method of measuring the quantity of light appears at first sight very simple, but I found on trial that it was attended with so many sources of error that I was afraid to depend upon it. Fortunately there is another method of much easier execution, which I found much more satisfactory.

The quantity of light given out during the combustion of coal-gas is very nearly proportional to its specific gravity; the heavier a gas is the slower does it issue from an orifice of a given diameter when propelled by a given force. I measured the time which a cubic foot of each gas took to issue from an orifice of $\frac{1}{30}$th of an inch, when propelled by a force such as to form a jet of flame, when lighted, of four inches in length, and I considered the goodness of the gas as proportional to this time: the result was as follows:—

<div style="text-align:center">

			′	″
1.	Glasgow gas, north	.	70	18
2.	...	south	.	60	9
3.	Skaterigg	. . . . . .	84	10
4.	Lesmahagow	. . . .	101	10
5.	Monkland	. . . . .	101	

</div>

Certainly, in a commercial point of view, the value of the gas (the price per cubic foot being the same in all) is exactly

proportional to the time that it takes to burn, because the consumption in a given time depends upon that time.

If, therefore, a thousand cubic feet of gas be charged 8s. on both sides of the river, it is clear that the consumers on the south side pay at the rate of 9s. 4d. per cubic foot, because they consume 7000 cubic feet in the same time that those on the north side consume 6000.

If Glasgow gas, Skaterigg gas and Lesmahagow gas are each charged at 8s. per 1000 cubic feet, the price paid by the consumers will be

		s.	d.
1000 feet of Lesmahagow gas		8	0
... Skaterigg gas		9	$4\frac{1}{4}$
... Glasgow gas :		11	$6\frac{1}{2}$
... ... south side river .		13	$5\frac{3}{4}$

XXV. *Note on the Decomposition of Carbonic Acid by the leaves of Plants under the influence of Yellow Light.* By JOHN WILLIAM DRAPER, *M.D., Professor of Chemistry in the University of New York.*

IN the year 1836 I discovered that the leaves of plants possess the quality of effecting the decomposition of carbonic acid under the influence of light which had passed through a solution of bichromate of potash, and which was so completely detithonized as to be unable to darken the chloride of silver. Under the same circumstances also, young plants which had been etiolated by germinating and growing in the dark, turned green in a few hours by the fixation of carbon, so as to constitute chlorophyll. Seeds also appeared to germinate without any difficulty, and eventually produced vigorous green plants.

These results were published in 1837 in the Journal of the Franklin Institute of Philadelphia*; I have likewise referred to them in the Phil. Mag. (Feb. 1840).

From time to time Mr. Hunt of Falmouth has called the accuracy of these experiments in question, and having made some investigations under the direction of the British Association, at its expense, he has published reports on the matter. It is the object of these publications to substantiate a doctrine which is derived from Sennebier, that the blue and violet chemical rays are the true cause of the decomposition.

That there might remain no reasonable doubt of the correctness of the views I had given, I repeated the experiment of

* I will take this opportunity to state that the experiments here alluded to, with some others, are now collected together, and are in the press. They will be in the hands of the English reader in a few weeks.

the decomposition in the prismatic spectrum. An account of this was read by Dr. Kane to the British Association at its meeting at Cork, and a memoir on the subject published in the Phil. Mag. (Sept. 1843). The result of these experiments completely proved the power of yellow light in producing the decomposition. The activity of the different rays seems to follow very closely their illuminating power. Experiments were also undertaken in Virginia, at my suggestion, by Dr. Gardner who resided there, with a view of determining directly whether the greening of leaves was produced by the same rays. As a matter of proof these experiments were not however absolutely required, because in assuming, as I have uniformly done, that the decomposition of carbonic acid and the production of chlorophyll are the same phænomenon, I merely follow all botanical writers. These experiments proved that the green colour is produced by yellow light, the other rays following in the order of their illuminating power (Phil. Mag., Jan. 1844). This is the same result as before; I have caused these experiments to be repeated in one of my laboratories in New York, and need scarcely add that they are verified.

It is therefore with no small surprise that I read in Mr. Hunt's recent treatise, entitled " Researches on Light," the following passage in reference to the memoir published in the Phil. Mag. (Sept. 1843), to prove that the yellow rays of light and not the blue and violet chemical rays brought about the decomposition in question. Mr. Hunt says, " Since this announcement, the very gloomy and uncertain state of the weather has almost entirely prevented my testing the correctness, or otherwise, of Dr. Draper's results. During a few faint gleams of sunshine I have repeated the experiments in Draper's own method, and I have found that *bubbles of air* have been liberated in the tubes under the influence of the yellow and red rays, but they have been *carbonic acid*. In the tubes which were placed in the blue and violet rays alone, a perfect decomposition had taken place, and the bubbles which were collected were *pure oxygen gas*."—*Researches*, p. 198.

Having recently, as the readers of this Journal know, expressed myself very plainly as to the character of Mr. Hunt's book, I do not wish to make any allusion here, except in so far as my experiments are called in question. It is true that I cannot understand how an experiment, which in America requires a most brilliant sun, and which involves some delicate manipulation, is to be made in a very " gloomy and uncertain state of the weather," and amid " a few faint gleams of sunshine." It would seem however that in England it can.

Neither do I understand how it is possible that the violet ray or any other ray can cause leaves to evolve *pure oxygen.* It has happened to me in certain investigations in vegetable physiology to make more than 200 analyses of gas from vegetable leaves.   They have never emitted pure oxygen, but a mixture of nitrogen and oxygen, and very often as much as fifty per cent. of the former gas.   This result is the same as has been obtained over and over again for more than fifty years.   M. Saussure, Dr. Daubeny, and all other competent writers are in agreement upon this point.

Neither can I understand how it is possible that an analysis can be made of the gas emitted by the violet ray.   With a brilliant sun and proper precautions taken to exclude extraneous light from the tubes, I have never been able with the violet light to cause leaves to evolve *a solitary bubble of any kind of gas whatsoever.*

I am therefore obliged to infer that there must be some error in Mr. Hunt's method of experimenting, or a want of care in his manipulation.   His results on the action of light upon plants are undoubtedly altogether erroneous.

The readers of the Philosophical Magazine who have perused my memoirs, will, I hope, have seen abundant reason to understand that no facts are ever there reported without a thorough evidence of their correctness.   In the limited space to which a periodical writer is restricted, much collateral evidence must be laid aside, or communications would become inordinately long.   So, in my memoir on the decomposition of carbonic acid, I might have added many other facts. Among the rest I might have given analyses of the gas emitted by the yellow ray.   I will conclude this paper by furnishing evidence on that point.

Five tubes, each three-eighths of an inch in diameter and six inches long, were inverted in a small trough of water containing carbonic acid, with which the tubes were also filled. Some blades of grass, nearly of the same size and volume, were placed in each tube.   This grass had been kept for two days in the dark, in a bottle filled with carbonated water.   During this time the film of air which envelopes all new leaves was removed, the grass became perfectly free from all adhering gaseous matter, and when in the carbonated water exhibited a dark green aspect.

I have previously found that leaves thus soaked emit, under the influence of the light, a larger amount of nitrogen than usual; this comes from the incipient decay of some of their nitrogenized constituents.   When, under these circumstances,

they are placed in the sunshine, this nitrogen comes off along with the gas liberated from the carbonic acid.

In the experiment I am now relating, a tube arranged like one of the foregoing five evolved, *in the open sunshine,* a certain volume of gas which was composed of

$$\left.\begin{array}{lll}\text{Oxygen} & . & . & . & 41\\ \text{Nitrogen} & . & . & . & 59\\ \text{Carbonic acid} & . & 00\end{array}\right\}100$$

The five tubes were placed in the spectrum in the following colours, and emitted the quantities of gas represented in the following table:—

1	Extreme red and red...	0·0
2	Orange and yellow......	19·8
3	Yellow and green	27·4
4	Blue........................	0·5
5	Indigo and violet	0·0

The gas in tube 2, which had been in the orange and yellow ray, was then washed with a solution of caustic potash. After this it still measured 19·8, containing therefore no perceptible quantity of carbonic acid. It was next examined for oxygen, and with the following result:—

Constitution of gas emitted by orange and yellow light:

$$\left.\begin{array}{lll}\text{Oxygen} & . & . & . & 8\text{·}00\\ \text{Nitrogen} & . & . & . & 11\text{·}80\\ \text{Carbonic acid} & . & 0\text{·}00\end{array}\right\}\ \text{or}\ \left\{\begin{array}{lll}\text{Oxygen} & . & . & . & 40\text{·}4\\ \text{Nitrogen} & . & . & . & 59\text{·}6\\ \text{Carbonic acid} & . & 00\text{·}0\end{array}\right.$$
$$\qquad\qquad\qquad\overline{19\text{·}80}\qquad\qquad\qquad\qquad\qquad\overline{100\text{·}0}$$

The gas evolved by the yellow and green rays was next analysed. Like the former, it underwent no diminution by washing with caustic potash. After this treatment it therefore measured 27·4, and on being examined for oxygen yielded as follows:—

Constitution of gas emitted by yellow and green light:

$$\left.\begin{array}{lll}\text{Oxygen} & . & . & . & 12\text{·}5\\ \text{Nitrogen} & . & . & . & 14\text{·}9\\ \text{Carbonic acid} & . & 00\text{·}0\end{array}\right\}\ \text{or}\ \left\{\begin{array}{lll}\text{Oxygen} & . & . & . & 45\text{·}6\\ \text{Nitrogen} & . & . & . & 54\text{·}4\\ \text{Carbonic acid} & . & 00\text{·}0\end{array}\right.$$
$$\qquad\qquad\qquad\overline{27\text{·}4}\qquad\qquad\qquad\qquad\qquad\overline{100\text{·}0}$$

In explanation of the large and variable amount of nitrogen occurring in these analyses, it will scarcely be necessary to remind the vegetable physiologist that it arises from the mode of conducting the experiment. In order to be absolutely certain that no atmospheric air infilmed the leaves, they were soaked in water, and then, when brought into the sunlight, the nitrogen which had accumulated on their tissues from incipi-

ent decay diffused out with the first portions of oxygen. As, therefore, more and more gas was evolved, the relative amount of the nitrogen diminished. Thus the reason that the third tube appeared to be richer in oxygen than the second, was owing to its containing more gas. Any person, however, who is familiar with the physiological action of leaves will understand these things without any further explanation.

June 15, 1844.

XXVI. *On a Combination of Lenses for the Photographic Camera Obscura.* By GEO. S. CUNDELL, *Esq.**

UNTIL within the last few years, the camera obscura has not been an instrument of sufficient importance to command much attention; photography, however, has conferred upon it a new character; and many attempts have recently been made to improve it, with various success.

To adapt the instrument to photography, the principal object is to obtain what is called " a *flat* field," or a picture which shall be in focus throughout, in the margin as well as in the centre; but, along with a flat and focal field, it is necessary to obtain a vivid and well-defined picture, with sufficient *light* to act energetically; and this last condition is especially required when living figures are to form any part of the picture.

It has been shown by Dr. Wollaston (and has been pointed out in a late number of the Philosophical Magazine†), that a lens of the meniscus figure, under certain conditions, will give a picture which, although not absolutely flat, is much more so than can be obtained by any other means; and, had he contemplated the adaptation of the instrument to photography, he would probably have made a small addition to it, similar to that about to be described.

All that his instrument requires, to make it a very perfect one, is a higher intensity of light; and (without impairing its other properties) it will be found, that that may be very efficiently given, by the following arrangement:—

Fig. 1.
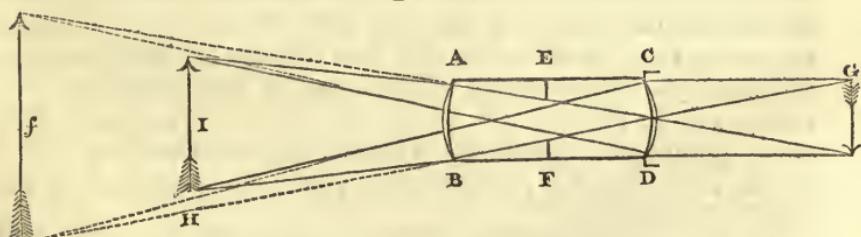
* Communicated by the Author.
† May 1844, S. 3, vol. xxiv. p. 321.

Instead of using a single lens, A B, with the concave side towards the radiant object G; let a second, and similar lens, C D, be placed before it, with the *convex* side outwards, and at a distance equal to one-third of its focus.

That such a combination will be, in a great degree, free from the aberration caused by the oblique rays, coming from the extreme parts of the picture, will be evident upon tracing the course of an oblique pencil, from its radiant point G, to the image of that point at H.

Of all the rays which fall upon the lens C D from the point G, those alone which fall between C and the centre of the lens are transmitted to H; these fall at a comparatively high, and equal, angle of incidence, upon *both* lenses; through which they pass nearly at right angles: while the more oblique rays (which would ruin the picture), falling between the centre of the lens and D, are all intercepted by the diaphragm E F.

But besides preventing much of the aberration of the oblique rays, the same combination has a tendency to correct the chromatic error, as will appear by observing the course of a single oblique ray G C.

Fig. 2.
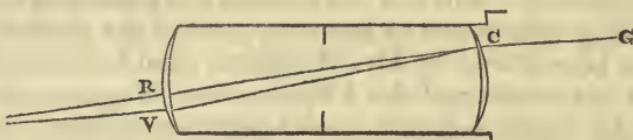
At the point C it will suffer dispersion, the red portion being refracted to R, and the violet to V. If there were no second lens, the rays would go on diverging and damaging the picture: but, being received upon the second lens, at unequal distances from its centre, the violet ray being furthest, it will receive a stronger refraction; and being besides more refrangible than the red, the spectral colours will now have a tendency to approximate, to reunite, and be neutralized.

It is scarcely necessary to show that the second lens will increase the intensity of the light; if there were only one lens, the focus would be at f (fig. 1); and the arrow at f would represent the size of the image; but the second lens being interposed, the convergency of the rays is quickened, and the image will be formed at I, of the size there represented. The same quantity of light is there condensed into about one-third of the area, and the photographic power is increased in the reverse proportion.

With the same aperture, the two lenses define nearly as well as when used singly: but, the light gained being very consider-

able, the aperture admits of contraction ; and, by reducing it to one-tenth of the compound focus, an image may be obtained of exquisite fineness.

Two lenses of 24 inches focus (3 inches diameter), placed 7 inches apart, have the same conjugate foci as a single lens of 13 inches ; and, with an aperture of 1·3 inch, the picture is beautifully defined : over 15° it is of a very high and uniform quality under a magnifier; and within 20°, there is little difference perceptible to the naked eye.

This combination is in strict accordance with the periscopic principle of Dr. Wollaston; and also, with that of the Coddington lens; I am not aware that it has yet been applied to the camera obscura, but it seems to be not less applicable to that instrument than to microscopes.

It might, perhaps, be improved by filling the space between the lenses with a blue coloured medium, of the same refraction as the glass; the focus would be shortened and the photographic power increased ; and the photographic rays associated with the red and yellow end of the spectrum being absorbed, would cease to interfere with the unity of the focus.

I avail myself of this opportunity to correct a mistake in an article on " the Calotype" in the Philosophical Magazine of May 1844. At p. 326, § 17, line second from the foot of the page, for "a pint of water" read *half* a pint (ten ounces).

London, July 15, 1844.

XXVII. *Additional Researches on the Electrolysis of Secondary Compounds. By* J. Frederic Daniell, *Esq., D.C.L., For. Sec. R.S., Prof. of Chem. in King's College, London, and* W. A. Miller, *Esq., M.D., Dem. of Chem. in King's College, London*[*].

THE authors of the following paper having agreed to work together upon the subject of the electrolysis of secondary compounds as opened by one of them, in two letters addressed to Dr. Faraday, and honoured with a place in the Philosophical Transactions for 1839 and 1840[†], have arrived at some results, which probably will not be without interest to the Royal Society, and which they have now the honour to communicate.

In the two papers just alluded to, the following points were

[†] An abstract of these papers was given in Phil. Mag. S. 3. vol. xvii. p. 349.—Edit.]

established.   *First.* When- aqueous solutions of the neutral metallic salts are exposed to the action of the voltaic current, they are invariably decomposed.   When the metal is one of that class which does not decompose water at ordinary temperatures, it is precipitated in the metallic state at the platinode : when it is of the class which does decompose water, its oxide, with an equivalent proportion of hydrogen, appears at the same electrode.   The acid at the same time is set free at the zincode, accompanied by an equivalent proportion of oxygen.   *Second.* When these results are compared with those of an independent voltameter in the same circuit, it is found that a certain proportion of the force which resolves a single equivalent of a *simple** electrolyte into its anion and cation, produces the resolution of a full equivalent of the *complex* electrolyte into a simple metallic cation and a compound anion. *Third.* When aqueous solutions of ammoniacal salts are electrolysed, similar results are obtained ; but instead of a simple metal being disengaged at the platinode, an equivalent of ammonia, accompanied in all cases by an equivalent of hydrogen, appears at the platinode.   *Fourth.* We are thus entitled to conclude that in the electrolysis of complex electrolytes, different elements travel together under the influence of the voltaic current as compound anions and cations, or as *iso-electric* bodies ; that is, groups which are equivalent to each other, and to simple ions, in their relations to the electric forces.

One of the most interesting results of this investigation, was the independent confirmation which it afforded of two celebrated hypotheses, viz. that of Davy regarding the constitution of aqueo-acids, and the general analogy in the constitution of salts, whether derived from oxyacids or hydro-acids ; and that of Berzelius concerning the constitution of ammonium.

It was with a view to extend our knowledge of such iso-electric groups, and to trace their connexion with chemical radicles, that we commenced our experiments.

Before we enter upon their detail, it will be best to make a few remarks upon the mode of measuring their results, which has been adopted.   It is founded upon the undoubted law of definite electric action ; and we have indifferently taken the amount of the ions disengaged at either or both the electrodes by the primary action of the current, or the secondary action of the elements.   But there is another mode of effecting this purpose, by a diaphragm cell, in which the products of electrolysis may be kept separate ; a method founded upon the

---

* By a simple electrolyte is meant one containing only two elementary substances, being the simplest form of matter capable of electrolysis.

hypothesis, that the voltaic decomposition of an electrolyte is not only effected by the disengagement of its anion and cation at their respective electrodes, but by the equivalent transfer of each to the electrodes; so that the measure of the quantity of matter translated to either side of the diaphragm might be taken as the measure of the electrolysis.

This was the mode which was necessarily adopted in the investigation before adverted to, where attempts were made to compare the results of the electrolysis of aqueous solutions of acids* and alkalies† with the simultaneous decomposition of saline solutions. One circumstance, however, was then overlooked, which it is necessary to attend to in making the comparison, viz. that the *disengagement* of whole equivalents of the ions at the electrodes is only accompanied, upon the hypothesis assumed, by the actual *transfer* of half an equivalent to *either* side. This will clearly appear from the following diagram.
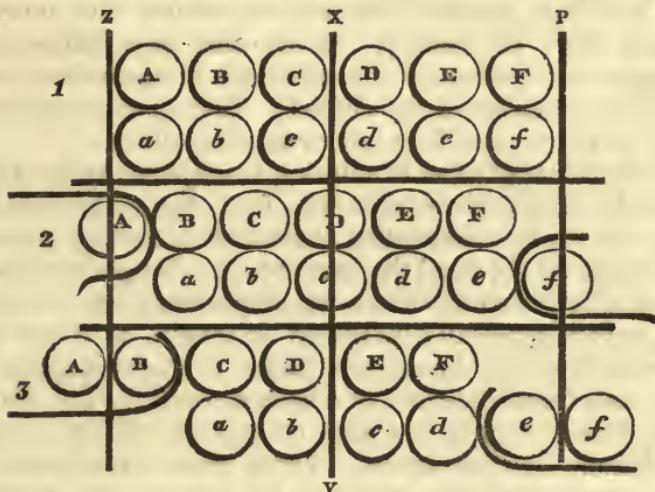
Let A, B, C, &c. in the preceding diagram represent a series of particles of chlorine, *a*, *b*, *c*, &c. a series of particles of potassium in combination with the particles of chlorine in the row above them, X, Y a central line or diaphragm, and Z and P the electrodes. If 1 represent the arrangement before the current passes, the particles A and *a*, B and *b*, &c. being combined to form compound particles of chloride of potassium, 2 would represent the arrangement after a single equivalent of each ion had been disengaged at the electrodes. Each particle would thus necessarily have moved forward half a step, combining with the next adjacent particle, so that B *a*, C *b*, D *c*, &c. would now form the chain between the electrodes. If another equivalent be now supposed to be set free at each

* First Letter, p. 108. † Second Letter, p. 216.

electrode, one particle of each ion would have passed the central line, one equivalent would thus be *transferred*, whilst two had been *disengaged*, as in fig. 3.

In the instances above referred to, the transfer of one quarter of an equivalent represented half of an equivalent of the hydrates respectively electrolysed. Little stress however need be laid upon this correction, inasmuch as we shall presently show that the hypothesis upon which it is founded, although generally received, is itself destitute of foundation.

1. Our first object was to ascertain more clearly than had yet been done, the influence of water in the aqueous solution of an electrolyte, by comparison with the results of the electrolysis of the same compound when in the state of igneous fusion. It had been already determined, that with regard to the *chlorides* no difference occurred, and that the amounts of chlorine evolved in the same circuit from *fused* chloride of lead[*], and from *dissolved* chloride of sodium and muriate of ammonia, were the same[†]. In the last case, ammonia and hydrogen were evolved at the platinode in equivalent proportions to the chlorine at the zincode. Here a compound cation (NH_4) was separated from an elementary anion.

When *nitrate of silver* in solution is subjected to electrolysis, the simple cation, silver, separates from the compound anion (NO_6), and upon substituting the *salt in a state of fusion*, for the solution, we obtained the same result. No gas was evolved, but *crystallized metallic silver* was deposited upon a silver platinode, which gradually increased in length, as it was slowly withdrawn from the liquid salt, just as in the analogous experiment with fused chloride of silver, devised by Dr. Faraday. Nitrous fumes were at the same time given off from the platinum zincode in abundance. From these experiments it is evident that neither the grouping of a compound cation, nor of a compound anion, is necessarily altered by water in the transit to their respective electrodes.

2. We now turned our attention to that most interesting group of salts, the *phosphates*, which has been so ably discussed by Prof. Graham[‡]; not without hopes of confirming by electrolysis the beautiful theory of their constitution, which he has derived from considerations purely chemical. *The double diaphragm cell*, which we chiefly employed in these experiments, is represented in the annexed woodcut.

A and B are the two halves of a stout glass cylinder, accurately ground so as to fit with shoulders liquid tight. C is a

---

[*] First Letter, p. 108.   [†] Ibid. p. 110.
[‡ Abstracts of Prof. Graham's researches on the phosphates will be found in Phil. Mag. S. 3. vol. iii. p. 451, 459.—Edit.]

hollow ring of glass, also ground on either side, with a flat
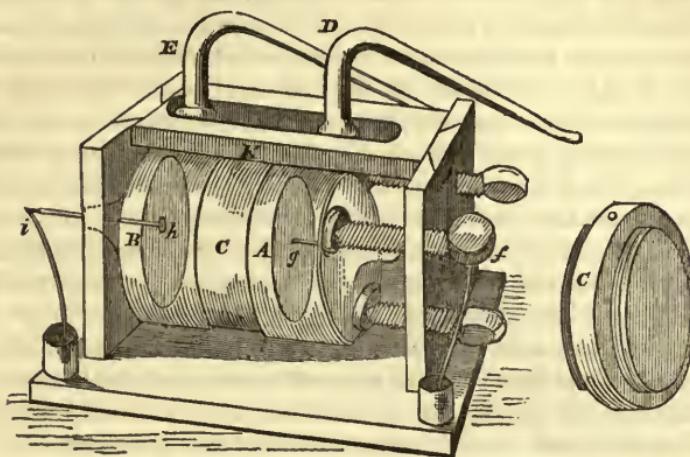
shoulder to fit against the two half cylinders, which are pressed home by the screws. Each side of the ring is furnished with a rim, which is grooved to admit of a thin piece of bladder being tied over it to form a kind of drum; at K is a small hole to admit of the cavity being filled with a liquid. D and E are two bent tubes, fitted to the two half cylinders for collecting the gases evolved in the experiments. *g* and *h* are two circular platinum electrodes connected by wires, *i, f* (passing through corks in the necks of the half cylinders), with the battery. The apparatus when adjusted forms three compartments, each of which may be filled with the same or a different liquid, and the whole may be supported on a light frame of wood.

We will not attempt to describe the particulars of every experiment referred to in the following pages, for their number is very great, and their details would be both tedious and useless; we will only select some of the principal, in the results of which we can trace no ambiguity; and have no doubt we shall obtain credit for every care in determining the purity of the substances which we employed, and in making the various analyses which were required.

(*a.*) A strong solution of *tribasic phosphate of soda and water* ($2NaO$, HO, P_2O_5) (rhombic phosphate) was placed in the platinode cell of the diaphragm apparatus; the centre cell and the zincode cell were both charged with a dilute solution of soda ($\frac{1}{48}$). The power of twenty cells of the small constant battery was transmitted through it, oxygen was evolved at the zincode, and in thirty-seven minutes 48 cubic inches of hydrogen were collected from the platinode; the experiment was then stopped and the solutions examined.

The liquid from the zincode cell was carefully neutralized with nitric acid, and then gave a copious yellow precipitate with nitrate of silver, soluble in nitric acid, and in ammonia, the well-known characters of the tribasic phosphate of silver $(3AgO, P_2O_5)$. The solution in the platinode cell had become much more alkaline than at first, but when neutralized with nitric acid, gave a similar yellow precipitate with nitrate of silver. There could be no doubt therefore that *tritoxyphosphion* (or the radicle of the tritohydrate of phosphoric acid) had travelled to the zincode. The experiment was more than once repeated with unvarying results.

(*b.*) A solution of *tribasic phosphate of soda* $(3NaO, P_2O_5)$ (sub-phosphate) was substituted in the same arrangement for the rhombic phosphate, and, after the action of the battery, on neutralizing the liquid in the zincode cell, a similar copious yellow precipitate was obtained from nitrate of silver.

(*c.*) To complete the series of tribasic phosphates in which different bases are substituted for each other, a solution of the *phosphate of soda, ammonia and water* $\left(\begin{matrix} NaO \\ NH_4O \\ HO \end{matrix} \right\} P_2O_5 \right)$ (microcosmic salt), was subjected in the same manner to electrolysis, the zincode cell being charged with solution of soda; the solution in the zincode cell became acid to the test of litmus, and produced an abundant yellow precipitate in solution of nitrate of silver; proving that it was the tribasic acid which had travelled as before.

(*d.*) A similar arrangement was made with a solution of *dibasic phosphate* (pyrophosphate) of soda $(2NaO, P_2O_5)$. Hydrogen and oxygen in equivalent proportions were respectively given out at the platinode and zincode, and the process was carried on to the same extent as before. The solution at the zincode after electrolysis was neutralized by nitric acid. When tested with solution of nitrate of silver, a copious white precipitate was formed, indicating that in this case *deutoxyphosphion* (or the radicle of the deuto-hydrate of phosphoric acid) had been transferred to the zincode.

(*e.*) A solution of *monobasic phosphate* (NaO, P_2O_5) (metaphosphate) was subjected to electrolysis under the same circumstances. The solution in the zincode cell was now found to be strongly acid instead of alkaline; when added to a solution of albumen it immediately coagulated it; and with nitrate of silver and nitrate of baryta it produced the characteristic gelatinous white precipitates, which are distinctive of the monobasic phosphates of those bases.

It was thus fully established by these experiments, and con-

firmed by their repetition, that the three hydrates of the phosphoric acid constitute three essentially distinct acids, which may be expressed as follows:—

$$H + P_2 O_6 \text{ proto-hydrate,}$$
$$H_2 + P_2 O_7 \text{ deuto-hydrate,}$$
$$H_3 + P_2 O_8 \text{ trito-hydrate,}$$
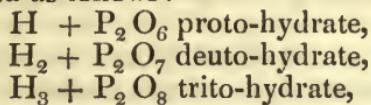
and that when disengaged from their corresponding saline compounds by the power of the voltaic current, they travel as three distinct *oxyphosphions* in the circuit. This view, which cannot be doubted as arising from the properties of the acids so disengaged, derives confirmation from the observation that the quantity of acid transferred from the monobasic phosphate of soda was so much greater for the same amount of hydrogen disengaged, than from the dibasic and tribasic salts of the same base.

We had thus the gratification of establishing by independent testimony the correctness of Prof. Graham's theory of the phosphates, and it is with pleasure we also observe that his view of the basic character of water in certain saline compounds* is confirmed by the results of the electrolysis of the three tribasic phosphates; from all of which the same acid was separated, notwithstanding that in two instances water obviously constituted one of the three equivalents of base. In our corresponding view, one of the three equivalents of metals was represented by hydrogen.

3. Next in natural order seems to stand the inquiry whether the salts of other tribasic acids would afford analogous results to those of the tribasic phosphates.

(*f.*) For this purpose we selected the *tribasic arseniate of potassa and water* ($KO, 2HO, As_2 O_5$) (binarseniate), the platinode cell was charged with a solution of the salt, and the zincode cell and connecting cell with solution of potassa. The combination was found to conduct very well, and oxygen and hydrogen were given off at their respective electrodes. The solution in the platinode cell assumed a grayish appearance, but there was no smell of arseniuretted hydrogen, nor was any arsenic deposited upon the electrode. The solution had become neutral to test paper. The liquid in the zincode cell was carefully neutralized by acetic acid, and afterwards produced in solution of nitrate of silver, the characteristic brick-red precipitate of the arseniate of silver ($3AgO, As_2 O_5$), so that there could be no doubt that the *tritoxarsenion* had travelled into the zincode cell.

The complicated secondary actions which take place in the

---

electrolysis of organic compounds, and which we hope to make the subject of future communications, induce us to abstain at present from extending this part of our inquiry to the organic acids.

4. An inquiry of considerable interest now claims our attention, viz. the electrolysis of salts, whose acids, having the same elementary radicle as some of those which have been already examined, are in an inferior degree of oxygenation: such are the arsenites and sulphites.

(*g.*) A solution of *arsenite of potassa* ($KO, As_2 O_3$) was carefully prepared (by dissolving single equivalents of arsenious acid and bicarbonate of potassa in boiling water), and placed in the platinode cell of the diaphragm apparatus; the connecting cell and the zincode cell were both charged with solution of potassa. During the electrolysis oxygen was given off at the zincode, but an equivalent of hydrogen was not obtained at the platinode, the electrode being covered with metallic arsenic, which was deposited in such abundance as speedily to render the liquid opake; there was no smell of arseniuretted hydrogen. This diffusion of the metal reduced by secondary action, throughout the solution, contrasts well with the compact manner in which metals are deposited, which, like copper from the neutral sulphate, are thrown down by direct voltaic action. The zincode solution was exactly neutralized by acetic acid; after which a yellow precipitate of the arsenite of silver was produced on adding nitrate of silver.

In this case, as in that of the arseniate just described (*3.f.*), free oxygen was evolved, but the corresponding hydrogen arising from the secondary action of the potassium at the other electrode was taken up in the reduction of the arsenious acid in contact with which it was evolved. No such reduction of the arsenic acid was effected.

It might have been anticipated that the oxygen in travelling with the arsenious acid to the zincode would have combined with it, and formed a portion of arsenic acid; but no such result took place: and it must be remarked, that two equivalents of oxygen instead of one would be required fully to oxidate the whole. We must, therefore, conclude that $As_2 O_3 + O$ constitute what we may for the present distinguish as a *suboxarsenion*.

The experiment was repeated, by charging all the three cells of the apparatus with the solution of the salt, when oxygen was given off from the zincode as before, and arsenious acid was deposited in white crystals.

(*h.*) The *carbonates* and *oxalates* may be compared together in the same point of view as acids of the same radicle in differ-

ent states of oxygenation. It has been already stated, that in the electrolysis of the former, carbonic acid and oxygen were found to be given off at the zincode in equivalent proportions to the alkali and hydrogen at the platinode; proving those salts to be *oxy-carbions* of the respective metals*. When oxalate of ammonia was treated in the same way, carbonic acid alone was evolved at the zincode, and ammonia with hydrogen at the platinode*. Here the *oxalion* (or sub-oxycarbion) contains the elements of two equivalents of carbonic acid $(C_2 O_3 + O) = 2(CO_2)$. In numerous repetitions which we now made of these experiments, we found that although in the electrolysis of oxalate of ammonia $(C_2 O_3 + O)$ was always exactly evolved, yet that it was not invariably in the form of carbonic acid, but in different proportions of oxalic acid with free oxygen and carbonic acid. It was thus proved that the burning of the oxalic acid into carbonic acid by its associated oxygen was a secondary process, and that the $(C_2 O_3 + O)$ which travelled in the circuit was in some way different from $2(CO_2)$.

(*i.*) A similar comparison was then made between the *sulphites* and *sulphates*. In the electrolysis of the latter, it has been shown that sulphuric acid with an equivalent of oxygen (which is evolved) travels to the zincode†. To determine the results of the former, all the cells of the diaphragm apparatus were charged with sulphite of potassa, and a voltameter was included in the circuit; hydrogen was given off from the platinode in exact proportion to that from the voltameter; but no oxygen was evolved from the zincode.

The experiment was by itself ambiguous, for it did not determine whether $(SO_2 + O)$ travelled as SO_3, or whether sulphuric acid was formed at the zincode by the secondary action of the oxygen upon the sulphurous acid. Judging from the analogy of the preceding instances, it is most probable that the oxygen with which the sulphurous acid is associated in the *sub-oxysulphion* is in a different state from that of the third equivalent in dry sulphuric acid, and that the sulphuric acid evolved at the zincode in the last experiment is the result of secondary action. Some sulphurous acid was evolved in the zincode cell, which doubtless arose from the action of the sulphuric acid upon the sulphite in contact with which it was produced.

(*k.*) The experiment was varied by substituting *hyposulphite of soda* ($NaO, S_2 O_2$) for the sulphite, and with analogous results. Hydrogen was given off abundantly at the platinode, but no gas was evolved at the zincode. There was a strong odour of sulphurous acid at the latter, with a gradual deposi-

tion of sulphur. This doubtless arose from the secondary action of the sulphuric acid, which was first formed upon the salt with which it was in contact, by which hyposulphurous acid was disengaged and immediately decomposed. The constitution of the hyposulphite being (NaO, $S_2 O_2$), that of the corresponding sub-oxysulphion would be $S_2 O_3$, which if burnt at the zincode would produce $S + SO_3$.

5. We now proceeded to apply the method of electrolysis to solve a problem of the same nature as that of the three hydrates of the phosphoric acid, viz. to determine the exact relations of the *yellow and red ferrocyanides of potassium.* It is well known that two views have been taken of the constitution of these two salts : the first represents the yellow salt as a *ferrocyanide of potassium* (K_2, Fe Cy_3), and the red salt as a *subsesquiferrocyanide* of potassium $K_3 + 2$(Fe Cy_3). The second view represents the two salts as compounds of those metals with two distinct radicles, the former denominated *ferrocyanogen* (Fe Cy_3), the latter *ferridcyanogen* (Fe$_2$ Cy_6). The ferrocyanogen forms a dibasic acid with hydrogen (H_2, Fe Cy_3), the salts of which are produced by the substitution of two equivalents of the metals for the two equivalents of hydrogen; and the ferridcyanogen forms with hydrogen a tribasic acid ($H_3 + Fe_2$ Cy_6), with a similar substitution of three equivalents of the metals in their different salts. The real chemical difference, therefore, between the yellow and red ferrocyanides of potassium, consists in the latter containing half an equivalent less of potassium than the former; and so with the other salts from the two classes the metallic elements are in the proportion of 1 to $1\frac{1}{2}$.

Now Mr. Porrett*, the discoverer of the hydroferrocyanic acid (or as he proposed to call it, the ferro-chyazic acid), long since determined that it travelled in the voltaic circuit by the electrolysis of its salts, to the zincode, the iron accompanying the cyanogen in this transfer, contrary to what might previously have been expected. The question which we now proposed to determine was, whether *ferridcyanogen* as well as *ferrocyanogen* would thus constitute a distinct anion under the directing influence of the battery.

Mr. Smee† has already determined that the subsesquiferrocyanide of potassium may be formed from the ferrocyanide by the action of the voltaic battery; and has even proposed the process for obtaining the red salt upon a large scale. He, however, explains the change by the action of *nascent oxygen*, which he supposes to be evolved at the zincode. He observes,

" that one equivalent of the yellow *ferrocyanate* is decomposed, the *free potash* travels one way, and the *hydroferrocyanic* acid the other. The oxygen unites with the hydrogen of the acid and sets ferrocyanogen at liberty; this again unites with an equivalent of the *ferrocyanuret of potassium* to form the *ferrosesquicyanuret.*" It is evident from this, that this gentleman entertains a very different notion of the steps of the electrolysis to that which we feel justified in supporting, and that therefore he has failed to place it in the point of view to which we propose to adhere.

(*l.*) We commenced our inquiry by carefully repeating Mr. Smee's experiment in a diaphragm cell, charged throughout with a strong solution of the *yellow ferrocyanide of potassium.* A voltameter was included in the circuit, and the power of twenty cells of the small constant battery was employed. Gas was evolved at the platinode, which was tested and found to be pure hydrogen; it was in equivalent proportion to the gases in the voltameter. No gas was at first evolved at the zincode, but the solution in that cell became gradually red, particularly at the surface. When 4 inches of hydrogen had been collected at the platinode a slight disengagement of gas commenced at the zincode, which continued, and proved to be oxygen, and was in the proportion of one-third of the gas from the platinode. After sixteen cubic inches had been collected from the platinode, we stopped the experiment and examined the results.

The platinode solution was much paler than before, and was alkaline. The zincode solution was deep green and acid, and the zincode plate was covered with a slight film of Prussian blue. Crystals of the red ferrocyanide separated from the zincode solution by spontaneous evaporation, and when the evaporation was complete, a radiated mass of a purplish-brown colour was obtained: on solution a small quantity of Prussian blue remained, and the solution had lost its acid reagency. The addition of acetate of lead to the solution produced but a scanty precipitate, proving the quantity of yellow ferrocyanide remaining in it to be but small.

The explanation of these results is, we think, sufficiently obvious. Ferrocyanogen was doubtless transferred to the zincode, and potassium to the platinode, the former combined with a portion of the ferrocyanide to form the subsesquiferrocyanide; the process being accelerated by the abstraction of a portion of the potassium passing over to the platinode, where by its reaction upon the water of the solution it was converted into potassa with the evolution of hydrogen. As the process proceeded more ferrocyanogen was given off at the zincode

than could be absorbed by the yellow salt; and it consequently began to react upon the water, becoming converted by the abstraction of its hydrogen into hydroferrocyanic acid, its oxygen being evolved. The acid reaction is thus accounted for, as well as the deposition of Prussian blue, from the well-known spontaneous decomposition of the acid.

(*m.*) This experiment was several times repeated with variation of the quantities, but with substantially the same results. The evolution of oxygen from the zincode was hastened by narrowing the dimensions of the electrode, which thus presented a smaller surface for the reaction of the ferrocyanogen upon the ferrocyanide. When the process was continued for a long time, yellow crystals of the ferrocyanide were formed in the platinode cell. Upon subsequent trial it was found that a strong solution of potassa produced a similar deposit, under ordinary circumstances, in a solution of the yellow salt. It will probably be objected, that although the red ferrocyanide is thus obviously formed by secondary action, yet where once formed it may be capable of decomposition as an electrolyte, in which ferridcyanogen acts as the anion. The following experiment however is decisive against such a view.

(*n.*) The diaphragm cell was charged throughout with a strong solution of *the red salt*, the purity of which had been tested. The solution was deep green by reflected light, but red by transmitted light. The power of the same constant battery was applied to it, and the platinode solution very quickly changed its colour and became yellow. When 5·75 cubic inches of hydrogen had been collected from the platinode the yellow colour was perfectly pure; the action was then stopped. The liquid at the zincode was of a very dark brown-green colour, and strongly acid, it had a pungent peculiar smell, the liquid soon became perfectly opake, and one cubic inch of oxygen was collected from the zincode. Though the colour of the two solutions in the electrode cells had undergone such marked changes, the colour of that in the intermediate cell remained unaltered. The liquid from the platinode cell yielded on evaporation tabular crystals of the yellow ferrocyanide, and when hydrate of potassa was added to the zincode solution, a yellow colour was developed.

Here then we obviously have the converse results of the last experiments; or the formation of ferrocyanide of potassium from subsesquiferrocyanide by the abstraction of half an equivalent of ferrocyanogen transferred from the platinode to the zincode.

(*o.*) Could a doubt remain with regard to the conclusion to be derived from these experiments, it must be removed by the

results of the following arrangement. The zincode cell was filled with a dilute solution of potassa, and the connecting cell with the same. The platinode cell was charged with a strong solution of the red ferrocyanide. This solution previously to the commencement of the experiment gave no blue solution with perchloride of iron. The liquid in the zincode and connecting cells speedily became yellow, and after a short time the solution in the platinode cell also became yellow. All the solutions were now found to contain the yellow ferrocyanide. Thus it was quite clear that no such compound as ferridcyanogen travelled from the red salt when exposed to electrolysis, but that the red salt was converted into the yellow by the abstraction of ferrocyanogen from the platinode, which ferrocyanogen being transferred to the zincode, there formed with the potassium presented to it by electrolysis the same yellow ferrocyanide.

In order to extend further our knowledge of the electrolysis of this class of secondary compounds, the following experiments were made :—

(p.) A diaphragm apparatus was charged with a strong solution of *cyanide of potassium* (K, Cy) in the platinode cell, and a solution of potassa in the zincode and connecting cells. The arrangement conducted well; gas was given off from both electrodes, and cyanide of potassium formed in abundance in the zincode cell. Hydrogen must therefore have been given off at the platinode by the oxidation of the potassium evolved, and oxygen at the zincode by the deoxidation of potassa under the influence of the current in the formation of the cyanide of potassium.

(q.) The apparatus was charged throughout with solution of cyanide of potassium, and subjected to the current. Gas was given off as before at the platinode ; but at first none from the zincode. The liquid in the zincode cell gradually became of a deep brown colour, and the electrode itself was coated with a deep brown matter, which was easily washed off. In the latter part of the operation gas was also evolved in small quantity at the zincode. The solution smelled strongly of hydrocyanic acid, of which, by the proper tests, it was found to contain a considerable quantity. There can be no doubt that the brown matter which was abundantly formed was *paracyanogen*, which is soluble in the alkaline solution of the cyanide of potassium, and which must have been produced either by the spontaneous decomposition of the hydrocyanic acid, or in a way something analogous to the formation of the red ferrocyanide of potassium from the yellow by the absorption of an additional quantity of ferrocyanogen.

(*r.*) A diaphragm apparatus was charged throughout with *sulphocyanide of potassium* (K, S_2 Cy). Upon the first application of the current the action was brisk, but soon declined, and when $5\frac{1}{2}$ cubic inches of gas had been collected from the platinode and $1\frac{1}{2}$ cubic inch from the zincode it ceased. There was a copious orange-coloured deposit in the zincode cell and upon the plate which interrupted the progress of the electrolysis; no particular odour was perceptible in either of the cells. The liquid filtered from the zincode became turbid by exposure to the air, and deposited more of the yellow compound. The clear liquid was again subjected to the current, which it transmitted readily, and with the same phænomena as before. There could be little doubt that *sulphocyanogen* was the anion transferred in this experiment. This part of our subject still presents a tempting field for investigation, from which however we were for a time diverted by an important class of facts which next fell under our observation; we have however since returned to the study, and are at present engaged in researches upon the cobaltocyanides, and other double cyanides, the results of which we hope ere long to lay before the Society.

<div align="center">[To be continued.]</div>

XXVIII. *New Properties of Surfaces of the Second Degree. By* John K. Ingram, *B.A., Trinity College, Dublin.*

To the Editors of the Philosophical Magazine and Journal.
    Gentlemen,

THE following extracts from a paper lately read before the Philosophical Society of Dublin may perhaps interest some of your readers. The theorems which I announce are, I believe, perfectly new; and some of them will be found, I think, to possess considerable geometrical elegance.

Being given two ellipsoids, reciprocal to each other (their common centre being the origin of transformation), let us describe the focal conics of one of them, the axes of which we may call *a*, *b*, *c*. Then in their planes describe the conics reciprocal to them. On these new conics let two cylinders be constructed with their generatrices perpendicular to the planes in which their bases lie. Those cylinders may be called the *cyclic cylinders* of the ellipsoid $\left(\dfrac{1}{a}, \dfrac{1}{b}, \dfrac{1}{c}\right)$: they possess many remarkable properties which are all derivable, by means of the polar transformation, from those of the focal conics.

Their situation in a given ellipsoid $\left(\dfrac{x^2}{a^2} + \dfrac{y^2}{b^2} + \dfrac{z^2}{c^2} = 1\right)$

may be easily understood. The equations of their bases are respectively as follows, it being supposed that $a > b > c$:—

$$\frac{(a^2-c^2)}{a^2 c^2} z^2 + \frac{b^2-c^2}{b^2 c^2} y^2 = 1,$$

$$\frac{(b^2-c^2)}{b^2 c^2} z^2 - \frac{(a^2-b^2)}{a^2 b^2} x^2 = 1.$$

The first is an ellipse, and lies in the plane of the least and mean axes of the surface, its major axis being coincident with the latter; the second is a hyperbola in the plane of the greatest and least axes of the surface, its transverse axis being situated on the latter. When one of these cylinders is given, the other may be constructed.

As two coaxal surfaces of the second degree are called confocal when they have the same focal conics, so they may be called *biconcyclic* when they have the same cyclic cylinders. [This term will not include all central surfaces of the second order the circular sections of which are coincident; those, for instance, which are similar and similarly placed, are excluded by the definition.] The cyclic cylinders will be found to have their cyclic planes coincident with those of the surfaces; and, as the focal conics are the limits of a series of confocal surfaces, to which they are ultimately reduced, the mean or least axis diminishing down to zero; so the cyclic cylinders are the limits of a system of biconcyclic surfaces, with which they ultimately coincide, the greatest or mean axis being indefinitely increased.

I subjoin a number of the properties of the cyclic cylinders and of biconcyclic surfaces, together with those of the focal conics and of confocal surfaces, from which they are respectively deduced.

Known Theorems.	*Derived Theorems.*
1. An ellipsoid being given, through any point may be described three surfaces of the second degree, coaxal and confocal with it; and of these one is an ellipsoid, and the other two hyperboloids of each kind.	1. An ellipsoid being given, three surfaces of the second degree may be described, touching a given plane, coaxal and biconcyclic with it; and of these one is an ellipsoid, and the other two hyperboloids of each kind.
2. And these three confocal surfaces are mutually orthogonal.	2. And the three lines drawn from the common centre to the three points of contact with the given plane are, two by two, at right angles.
3. If any point be made the common vertex of two cones,	3. If a plane be drawn cutting a surface of the second

one standing on a focal conic of a surface of the second degree, and the other enveloping that surface, these two cones will have the same principal axes and the same focal lines.

4. If any point be made the common vertex of two cones standing on the two focal conics of a surface of the second degree, these two cones will have the same principal axes and the same focal lines.

5. If any point be made the vertex of a cone circumscribing an ellipsoid, the tangent plane applied at the point to a coaxal and confocal ellipsoid described through it, is a principal plane of that cone.

6. And the tangent planes applied at the same point to the other two coaxal and biconcyclic surfaces of the second degree described through it, are the other two principal planes of that cone.

7. If any point on an ellipsoid be made the vertex of a cone standing on one of its focal conics, the tangent plane to the surface at the point will be a principal plane of that cone.

degree and one of its cyclic cylinders in two curves, and the centre of the surface be made the common vertex of two cones, passing respectively through those two curves, the two cones will have the same principal axes and the same cyclic planes.

4. If a plane be drawn cutting the cyclic cylinders of a surface of the second degree in two curves, and the centre of the surface be made the common vertex of two cones passing respectively through those two curves, the two cones will have the same principal axes and the same cyclic planes.

5. If a plane be drawn cutting an ellipsoid, and a coaxal and biconcyclic ellipsoid be described touching this plane, the line joining the centre to the point of contact is a principal axis of the cone having its vertex at the centre, and passing through the plane section of the first ellipsoid.

6. And if the other two coaxal and biconcyclic surfaces of the second degree, tangent to that plane, be described, the lines drawn from the centre to their two points of contact with it are the other two principal axes of the same cone.

7. If a tangent plane to an ellipsoid intersect one of its cyclic cylinders, the cone having its vertex at the centre of the surface, and passing through the section of the cylinder has for one of its principal axes the right line drawn to the point of contact of the tangent plane.

8. There are four points of an ellipsoid (the umbilics namely), which are the vertices of cones of revolution standing on one of its focal conics.

9. The locus of such points in a system of coaxal and confocal ellipsoids is the conjugate focal conic, which is a hyperbola.

10. The focal hyperbola is also the locus of the vertices of cones of revolution enveloping a given ellipsoid.

11. The intersection of three rectangular planes, tangent respectively to three coaxal and confocal ellipsoids, moves on a sphere concentric with the ellipsoids.

12. The difference of the squares of perpendiculars let fall from the common centre on parallel tangent planes to two coaxal and confocal ellipsoids is constant.

13. If through a line lying in the plane of one of the focal conics of a system of coaxal and confocal ellipsoids, a tangent plane be applied to each of them, the locus of the points of contact is a circle.

8. There are four tangent planes to an ellipsoid intersecting one of its cyclic cylinders in curves which lie on cones of revolution, having their common vertex at the centre of the surface.

9. The series of such tangent planes to a system of coaxal and biconcyclic ellipsoids envelope the conjugate cyclic cylinder, whose base is a hyperbola.

10. The same cylinder is the envelope of a series of planes cutting out of an ellipsoid sections which lie on cones of revolution, whose common vertex is the centre of the system.

11. If three radii, which are two by two at right angles, be drawn from the common centre of three coaxal and biconcyclic ellipsoids, and through the points where these radii respectively meet the three surfaces, a plane be made to pass, this plane will envelope a sphere concentric with the ellipsoids.

12. The difference of the squares of the reciprocals of coincident radii in two coaxal and biconcyclic ellipsoids is constant.

13. If a line be parallel to the axis of one of the cyclic cylinders of a system of coaxal and biconcyclic ellipsoids, and at the points where it meets each of these surfaces, tangent planes be applied to them, these planes will envelope a cone of the second degree.

When the ellipsoid to which a cyclic cylinder belongs be-

comes one of revolution, the cyclic cylinder itself degenerates into two planes, analogous to two right lines, possessing remarkable properties in the plane conics.

These right lines, which correspond in the geometry of the plane to the *cyclic arcs* of a spherical conic, were discussed in the same paper from which the above theorems are extracted, and were shown to be derivable from the foci of the reciprocal conic by a polar transformation.

<div align="left">Trinity College, Dublin,</div> <div align="right">John. K. Ingram.</div>
February 7, 1844.

XXIX. *On some of the Salts of Meconic and Komenic Acids.* By John Stenhouse, *Ph.D.*[*]

THE meconic acid employed in the following experiments was made by Robiquet's process, as modified by Gregory. The crystallized acid had only a slightly yellowish shade, and when heated on platinum foil it disappeared without leaving any residue When dried at 212° F. it was subjected to analysis. 0·4533 gram. of acid gave 0·696 of carbonic acid and 0·0847 of water.

		Calculated numbers.		
				Per cent.
Carbon .	42·45	Carbon . 14 =	42·460
Hydrogen	2·07	Hydrogen 1 =	1·979
Oxygen .	55·48	Oxygen . 11 =	55·561
	100·00			100·000

Meconate of Lead.

When meconic acid is added in slight excess to a solution of neutral acetate of lead, it causes a bulky flocculent precipitate, which is nearly white, with only a slight shade of yellow. The precipitate is insoluble in either hot or cold water. When washed and dried at 212° F. it was subjected to analysis.

	Gram.						Per cent.
I.	0·4707	gave	0·0348	lead and	0·262	oxide =	63·62
II.	0·420	...	0·0495	...	0·213	...	= 63·41
III.	0·3952	...	0·0543	...	0·1927	...	= 63·56
IV.	0·4508	...	0·0385	...	0·2435	...	= 63·21
V.	0·4987	...	0·1043	...	0·203	...	= 63·23

I. 0·367 grm. of salt ignited with chromate of lead gave 0·215 of carbonic acid and 0·0211 of water.

II. 0·554 grm. gave 0·331 of carbonic acid and 0·0346 of water.

---

[*] Communicated by the Chemical Society; having been read April 1, 1844. Dr. Stenhouse's paper On the Products of the Distillation of Meconic Acid, will be found in our preceding volume, p. 128.

	I.	II.	At.	Calculated numbers.		Per cent.
C	$=16{\cdot}19$	$16{\cdot}52$	14 Carbon	$=1070{\cdot}09$		$16{\cdot}23$
H	$={\cdot}63$	${\cdot}69$	3 Hydrogen	$=37{\cdot}44$		${\cdot}57$
O	$=19{\cdot}78$	$19{\cdot}39$	13 Oxygen	$=1300{\cdot}00$		$19{\cdot}72$
PbO	$=63{\cdot}40$	$63{\cdot}40$	3 PbO	$=4183{\cdot}5$		$63{\cdot}48$
	$\overline{100{\cdot}00}$	$\overline{100{\cdot}00}$		$\overline{6591{\cdot}03}$		$\overline{100{\cdot}00}$

The salt employed in these determinations was made at three different times; that used for the first and second determinations was washed with hot water, the others were washed with cold water. The salt is evidently tribasic with 2 atoms water, and its formula is consequently $\overline{Me} + 3PbO + 2$ aq. I have been quite unable to obtain Robiquet's salt: $\overline{Me} + 2PbO +$ aq.

When meconate of ammonia is added to a solution of acetate of lead, rendered also slightly alkaline by ammonia, still more basic salts are formed. They fall as deep yellow insoluble precipitates. In the course of three trials I was quite unable to procure them of definite composition. The mean of several determinations of one quantity gave $68{\cdot}38$ per cent. oxide of lead, a second gave $74{\cdot}76$, and a third $71{\cdot}07$ per cent. The first quantity was washed with hot, and the other two with cold water.

Meconic Acid and the Persalts of Iron.

When meconic acid is added to a solution of any of the persalts of iron, the liquid, as is well known, immediately assumes a blood-red colour; but even though very concentrated it may be kept for any length of time without its giving any precipitate. With an alcoholic solution of meconic acid the result is precisely similar.

If, however, a tolerably concentrated aqueous solution of meconic acid is first neutralized with ammonia, and then treated with a slight excess of very neutral persulphate of iron, after standing for a few hours a fine bright crimson precipitate appears in considerable quantity. This powder, which is not at all crystalline, should be collected on a filter and washed with cold water till the liquid which passes off is but slightly coloured, and ceases to give any indication of containing sulphuric acid. The red precipitate is not very soluble in cold water or alcohol, but dissolves very readily either in an acid solution or in hot water. When its solution is treated with potash or soda it is instantly rendered colourless, and the smell of ammonia becomes very perceptible. The iron is precipitated in reddish flocks, evidently in the state of peroxide, for when redissolved in muriatic acid, and tested with red prussiate of potash, not the slightest blue tint is perceptible.

If a solution of the powder which has been decolorized by potash be cautiously neutralized by muriatic acid, the red colour reappears, but is destroyed by the addition of more acid. The iron is not precipitated by ammonia, even when added in great excess, and the colour is only changed from a blood-red to a deep reddish yellow. Hydrate of lime also destroys the colour, precipitates the iron as peroxide and evolves ammonia. If the red powder is dried at an ordinary temperature it retains its bright colour, but if it is heated as high as 212° F. it becomes reddish brown, losing its lustre. If first dried at an ordinary temperature, it may be heated as high as 212° F. without any reduction of its oxide. But if its solution is kept for some time at 120° F., it is partially reduced; and if boiled for an hour or so its red colour disappears, and the peroxide of iron it contains is wholly reduced to the state of protoxide. When dried at 212° F., and subjected to analysis, the determinations of the iron double salt were as follows :—

 I. 0·497 gave 0·1125 oxide = 22·63 per cent.
 II. 0·403 ... 0·0905 ... = 22·45
 III. 0·470 ... 0·1105 ... = 23·51
 IV. 0·472 ... 0·110 ... = 23·30
 V. 0·5034 ... 0·117 ... = 23·24
 VI. 0·2399 ... 0·0557 ... = 23·22
 VII. 0·547 ... 0·126 ... = 23·03 Mean 23·05 per cent.
 VIII. 0·5741 ... 0·1387 ... = 24·15
 IX. 0·3935 ... 0·096 ... = 24·39

The last two determinations of the iron are omitted, as probably too high in calculating the mean quantity, — 23·05 per cent.

 I. 0·3702 gave 0·4122 carbonic acid and 0·0705 water.
 II. 0·2390 ... 0·27 ... ... 0·051 ...
 III. 0·4130 ... 0·468 ... ... 0·0905 ...
 IV. 0·4983 ... 0·5665 ... ... 0·113 ...
 V. 0·3662 ... 0·4165 ... ... 0·0760 ...

 I. 0·7052 grm. dissolved in alcohol acidulated with muriatic acid gas and precipitated with an alcoholic solution of chloride of platinum, gave 0·171 of platinum = 4·22 per cent. of ammonia, or 3·49 of nitrogen.

 II. 0·4223 grm. of salt gave 0·1015 of platinum = 4·18 ammonia, or 3·45 nitrogen. Mean 3·47 per cent. nitrogen.

	I.	II.	III.	IV.	V.
Carbon =	30·78	31·23	31·33	31·43	31·44 per cent.
Hydrogen =	2·11	2·37	2·43	2·51	2·30
Oxygen =	40·57	39·81	39·70	39·52	39·72
Nitrogen =	3·49	3·49	3·49	3·49	3·49
$Fe_2 O_3$ =	23·05	23·05	23·05	23·05	23·05
	100·00	100·00	100·00	100·00	100·00

The salt employed in these determinations was made at five different times. The compound is evidently a double salt, consisting of meconate of iron and meconate of ammonia. The discrepancies in the determinations are probably owing to the circumstance that one of the constituent salts is more soluble in the wash-water than the other.

Iron Salt from Ethereal Solution of Meconic Acid.

A cold ethereal solution of meconic acid was made by agitating the acid, in the state of fine powder, in a stopped bottle with anhydrous æther. A considerable quantity of æther must be employed, as the meconic acid is but very slightly soluble in that liquid. The saturated solution was then filtered into a second bottle, containing an ethereal solution of perchloride of iron. This was made by dissolving neutral perchloride of iron, which had previously been gently evaporated to dryness, in anhydrous æther. When mixed, the two liquids became of a deep red colour, and in a few minutes a quantity of reddish-brown flocks precipitated. These were collected on a filter and washed with æther till any adhering chloride of iron was removed. When dried they formed a reddish-brown powder. This can only be obtained by the use of anhydrous æther; if it contains any water, no precipitate falls, and the salt is condensed in blood-red drops on the sides of the bottle. The presence of alcohol is equally objectionable. After the salt has been dried it becomes much less soluble than before. It dissolves pretty readily in cold water and alcohol, but more readily in hot water, and the addition of a little acid greatly increases its solubility. The colour of its solution is brownish-red. When it is treated with a slight excess of potash or soda it is instantly decolorized, and the iron precipitated in the state of peroxide. The iron of this compound is not precipitated by an excess of ammonia, and its colour is only changed to a deep yellowish-red. Dried at 212° F.,—

   I. 0·3826 salt gave 0·117 peroxide $= 30·58$ per cent.
  II. 0·2705   ...   0·082   ... $= 30·31$ ...
 III. 0·2307   ...   0·0721   ... $= 31·25$ ...
 IV. 0·2593   ...   0·0803   ... $= 30·96$ ...
  V. 0·3131   ...   0·972   ... $= 31·03$ ...

                                 Mean 30·82   ...

   I. 0·2747 salt gave 0·261 carbonic acid and 0·0475 water.
  II. 0·441   ...   0·41   ...   ...   0·0755 ...
 III. 0·345   ...   0·325   ...   ...   0·054 ...,

			I.	II.	III.
Carbon	.	=	26·27	25·70	26·03
Hydrogen	.	=	1·92	1·90	1·74
Oxygen	.	=	40·99	41·58	41·41
$Fe_2 O_3$	.	=	30·82	30·82	30·82
			100·00	100·00	100·00

The salt employed in these determinations was prepared at three or four different times; but though the results agree pretty closely, I have not been able to fix upon a probable formula for the salt.

When meconic acid is agitated in the cold with freshly precipitated peroxide of iron no effect is produced, and if a slight heat is applied, only a very partial combination is effected. If the heat is increased, though much under 212° F., a large portion of the peroxide is dissolved by the acid; but if the compound is examined, much of the iron has been reduced to the state of protoxide. If the liquid is filtered, it deposits on cooling a quantity of a reddish powder, which consists of meconic acid in combination with a mixture of peroxide and protoxide of iron; sometimes the oxide amounts to as much as 34 per cent.

Komenic Acid.

This acid is easily prepared by boiling meconate of lime in a great excess of strong muriatic acid. This removes almost the whole of the base, and converts the meconic into the komenic acid. When the solution cools the impure acid is deposited in hard reddish coloured crystals. The best way of purifying the komenic acid is to dissolve it with the assistance of heat in a slight excess of a concentrated solution of either potash or soda, and to filter the liquid while hot. This removes any portion of lime which the komenic acid may still have retained. On the cooling of the solution the komenate of potash crystallizes in white mammillated masses. These should be washed with a little cold water till the highly-coloured mother liquor is entirely removed. The salt is then easily decomposed by boiling it with an excess of pure muriatic acid, from which the komenic acid may be freed by two or three crystallizations. Komenic acid, even when pure, has usually a slight shade of reddish-yellow. This may be almost entirely removed by digesting the acid with purified animal charcoal. When heated on platinum foil it does not leave any residue.

Komenate of Ammonia.

When a slight excess of ammonia was added to a hot solution of komenic acid it immediately became of a pale yellow

colour. The liquid was concentrated in vacuo over sulphuric acid. The salt crystallized partly in a confused mass and partly in short four-sided prisms. It had only a slight shade of yellow, and its powder was very white.

When dried at 212° F. :—

I. 0·9925 grm. lost 0·0945 = 9·04 per cent.
II. 1·275   ... lost 0·123 = 9·035 ...

I. 0·3832 grm. of salt burned with oxide of copper gave 0·5815 carbonic acid and 0·139 water.

II. 0·3119 grm. of salt gave 0·4723 carbonic acid and 0·1168 water.

0·4015 grm. of salt dissolved in alcohol acidulated with muriatic acid gas gave 0·22 grm. of platinum = 9·75 per cent. ammonia or 8·04 per cent. nitrogen.

	I.	II.	At.	Calculated numbers.	Per cent.
Carbon .	41·96	41·86	12 Carbon	= 917·22	42·04
Hydrogen	4·03	4·25	7 Hydrogen =	87·357	4·01
Oxygen .	45·97	45·85	10 Oxygen	=1000·000	45·83
Nitrogen .	8·04	8·04	1 Nitrogen	= 177·04	8·12
	100·00	100·00		2181·617	100·00

The salt therefore is the acid komenate of ammonia with 1 atom of water of crystallization. Its formula is $\overline{K}O + NH_3 + HO + 1$ at. aq.

Komenate of Lead.

When neutral acetate of lead was added to a solution of komenic acid it caused a whitish granular precipitate, which immediately redissolved in the excess of acid present in the liquid. This precipitate reappeared and remained permanently on the addition of more acetate of lead. The precipitate, which acquired a slightly yellow colour, was collected on a filter and washed. When dried at 212° F. it was subjected to analysis.

I. 0·5316 grm. of salt gave 0·1389 of metallic lead and 0·1295 oxide = 58·50 per cent. oxide.

II. 0·411 grm. gave 0·103 lead and 0·1295 oxide = 58·50 per cent. of oxide.

I. 0·3683 salt when burned with chromate of lead gave 0·255 carbonic acid and 0·0385 water.

	At.	Calculated numbers.	Per cent.
Carbon = 19·14	12 Carbon .	= 917·220	19·28
Hydrogen = 1·16	4 Hydrogen	= 49·918	1·05
Oxygen = 21·20	10 Oxygen .	=1000·000	21·03
PbO . = 58·50	2 Oxide of lead =2789·000	58·64
100·00		4756·138	100·00

This salt therefore is bibasic with 2 atoms water, and its formula is $C_{12}H_2O_8 + 2PbO + 2aq = \overline{K}O + 2PbO + 2aq$.

The same salt is also formed when komenate of ammonia is dropt into a solution of acetate of lead. Komenic acid appears therefore to form only one lead salt. This result is rather unexpected, as when komenic acid is neutralized with ammonia it forms a second silver salt.

The salt made from acetate of lead and komenate of ammonia was also analysed.

I. 0·5615 grm. of salt gave 0·2025 of metallic lead and 0·111 of oxide = 58·61 per cent. oxide.

II. 0·8745 grm. of salt gave 0·2075 of lead and 0·29 oxide = 58·72 per cent. oxide.

Komenate of Copper.

When a hot solution of komenic acid is added to sulphate of copper it changes its colour from blue to dark green, and after standing a few minutes, a crystalline precipitate, of the colour of Schweinfurth green, is slowly deposited. The crystals, though small, are very distinct; they are elongated pyramids of a bright green colour, but the colour of their powder is much lighter. When dried at 212° F. :—

I. 0·35 gr. of salt gave 0·117 oxide = 33·42 per cent.
II. 0·2865   ...   ...   0·0955 ... = 33·33   ...
                                          33·37 mean.

I. 0·4204 grm. of salt burned with oxide of copper gave 0·4664 carbonic acid and 0·0685 water.

II. 0·4636 grm. of salt gave 0·5225 of carbonic acid and 0·0775 water.

	I.	II.	At.	Calculated numbers.		Per cent.
C .	30·67	31·22	12	Carbon . . .	917·22	31·00
H .	1·81	1·85	8	Hydrogen . .	49·918	1·68
O .	34·15	33·56	10	Oxygen . . .	1000·000	33·81
CuO .	33·37	33·37	2	Oxide of copper	991·400	33·51
	100·00	100·00			2958·538	100·00

This is evidently a bibasic salt with two atoms of water. Its formula is $\overline{K}O + 2CuO + 2aq$.

When komenate of ammonia is employed instead of komenic acid the same salt is also formed, but it then falls as a copious flocculent precipitate of a greenish yellow colour. 0·5507 grm. of salt gave 0·184 of oxide = 33·41 per cent.

Komenic acid also changed a solution of acetate of copper to a green colour, but produced a very scanty precipitate. The addition of ammonia did not increase the quantity of the precipitate, but gave the solution a rich blue colour.

Komenate of Silver.

Komenic acid forms two silver salts, both of which have been examined by Professor Liebig.

The monobasic salt, which is a white flocculent precipitate, is made by adding komenic acid to a solution of nitrate of silver.

I. 0·2395 grm. of salt gave 0·590 of silver = 43·64 per cent.
II. 0·4715 ... ... 0·103 ... = 43·98 ...

The calculated number is 43·93, and the formula of the salt $\overline{\text{KO}}$ AgO + aq.

The yellow salt is formed by adding komenate of ammonia to nitrate of silver.

I. 0·313 grm. gave 0·1825 silver = 62·62 per cent. oxide.
II. 0·558 ... 0·325 ... = 62·46 ...

The calculated number is 62·48, with which I need scarcely add that Professor Liebig's results approach very closely.

XXX. *On the Reduction of the Salts of Peroxide of Iron by means of Vegetable Substances.* By JOHN STENHOUSE, Ph.D.[*]

IN the course of some experiments I had recently occasion to observe that some of the most common vegetable substances reduce the persalts of iron with very great facility. A quantity of common meadow grass was immersed in a tolerably dilute cold solution of persulphate of iron. In the course of half an hour, when tested with red prussiate of potash, the liquid gave a deep blue precipitate, indicating the formation of a considerable quantity of protosulphate of iron. After three days the solution was again tested, when the iron was found to have been completely reduced to the state of protoxide. Several other kinds of green vegetable matters, such as twigs of trees and shrubs, produced similar results; as did meadow-hay, chips of wood and sawdust. Peat and wood charcoal, of all the substances which I tried, appeared to be those which effected the reduction of the persulphate with the greatest rapidity. A solution was completely decomposed by either of them in less than twenty-four hours; but with the assistance of heat the reduction was effected in an hour or two. Sugar, starch, gum, spirits of wine, oil of turpentine and pieces of paper, though they produced no effect in the cold, at a boiling temperature also caused a partial reduction of the salt. The substances previously enumerated operated exactly on solutions of the perchloride of iron as on those of the persulphate.

[*] Communicated by the Chemical Society; having been read April 1, 1844.

XXXI. *On the Hydrate of the Oil of Laurel Turpentine.* *By* John Stenhouse, *Ph.D.**

A FORMER communication contained a short account of a volatile oil called laurel oil of Guiana, said by Professor Christison to be obtained by incisions from a species of ocotea. It was found to consist of two isomeric oils, having the same composition, $C_5 H_4$, as oil of turpentine†.

A quantity of this oil was mixed with some spirits of wine and a little nitric acid, exactly in the way recommended by M. Wiggers for procuring the hydrate of oil of turpentine. After it had stood for some months, a large portion of the oil was converted into a crystalline mass of a deep yellow colour. By dissolving it and repeatedly crystallizing it out of spirits of wine, it was readily procured in fine large rhombic prisms, which were perfectly white, and had neither taste or smell. These crystals melted at 257° F., and when heated began to sublime at about 266° F. When dried at 212° F. they were subjected to analysis in the usual way.

I. 0·2615 grm. substance gave 0·662 of carbonic acid and 0·275 water.

II. 0·3068 grm. gave 0·780 of carbonic acid and 0·321 water.

| | I. | II. | Calculated numbers. | |
			At.	Per cent.
Carbon	69·99	70·30	5 Carbon	= 70·19
Hydrogen	11·68	11·62	5 Hydrogen	= 11·44
Oxygen	18·33	18·08	1 Oxygen	= 18·37
	100·00	100·00		100·00

It is evident from these analyses that the crystals of hydrated laurel oil are isomeric and isomorphous with those of the hydrates of the oils of turpentine and cedar. Their most probable formula is $C_5 H_4 + HO$, or one atom oil and one atom water. This is rendered still more probable by the following experiment. When a portion of these crystals were distilled with anhydrous phosphoric acid they yielded a colourless oil, lighter than water, which had the same taste and smell as laurel oil, and appeared to be that oil regenerated. Unfortunately, from the small quantity of the crystals in my possession, I was unable to verify this by analysis.

The crystals also dissolved when gently heated in strong sulphuric acid, to which they gave a red colour. They evolved balsamic vapours which strongly reddened litmus. The addition of water precipitated the oil changed into a tenacious resin.

* Communicated by the Chemical Society; having been read April 1, 1844.

† See Phil. Mag. S. 3. vol. xx. p. 273.

XXXII. *On East Indian Grass Oil.*
By JOHN STENHOUSE, *Ph.D.**

I AM indebted to the kindness of Professor Christison for a quantity of an East Indian grass oil, which forms the subject of the present notice. This oil is said to be the produce of *Andropogon Ivaracusa,* and is I believe what is commonly called the oil of Namur, though Dr. Royle affirms that the grass oil of Namur is derived from the Andropogon (*Calamus aromaticus*), the true spikenard of the ancients.

The oil which I have subjected to examination has a very fragrant aromatic odour, slightly resembling that of otto of roses, but not nearly so rich. Its taste is sharp and agreeable, approaching that of oil of lemons. It has a deep yellow colour, and evidently contains a good deal of resinous matter, which induces me to suspect that it is pretty old. It is lighter than water, and quite neutral to test paper.

When mixed with water and subjected to distillation, it left nearly one-half of its bulk of a fluid resin. The oil which passed into the receiver was colourless, but its smell was not quite so agreeable as before distillation, and the same was the case with the resin in the retort. The oil was rendered anhydrous by treating it with chloride of calcium and again rectifying it.

It began to boil at 297° F., but the boiling gradually rose to 320° F., when it became stationary for some time, and then continued rising. When subjected to analysis

0·3465 gr. gave 1·061 of carbonic acid and 0·3575 water.

Carbon	83·76
Hydrogen	11·46
Oxygen	4·78
	100·00

As the analysis evidently showed that the oil consisted of a carburetted hydrogen mixed with a little of an oxygenated oil, a quantity of the rectified oil was treated with small pieces of sodium. These immediately caused an evolution of hydrogen, but this ceased in a short time, and the bits of sodium which had not dissolved remained quite bright, while a small quantity of a reddish coloured resin was also formed. The oil was then poured off the sodium and rectified. When subjected to analysis

0·2675 gram. gave 0·855 of carbonic acid and 0·277 water.

	At.	Calculated numbers.
Carbon .	88·37	5 Carbon = 88·46
Hydrogen	11·50	4 Hydrogen = 11·54
	99·87	100·00

The grass oil consists, therefore, of a carburetted hydrogen, having the same composition, $C_5 H_4$, as the numerous other oils of which oil of turpentine is the type, together with a small quantity of one or more oxygenated oils. The affinity which this grass oil has for oxygen appears to be very considerable; for even after being purified, the carburetted hydrogen part of the oil cannot be distilled without a little of it being converted into a resin. The resinous matter, of which the oil contains such a large quantity, is, I should think, therefore most probably the effect of oxidation.

XXXIII. *Observations on the Pharmaceutical and Chemical Characters of the Peruvian Matico. By* JOHN F. HODGES, *M.D.**

ABOUT five years ago I received a parcel of the leaves of the Matico from a friend who had resided many years in Peru. He informed me that they were universally regarded by the native practitioners of that country as a most valuable remedy in various diseases, and also related several marvellous stories, current among the Indians and European settlers, of their power in arresting the most violent hæmorrhages, particularly those connected with the bladder and urinary organs. At the time I made a few experiments with the leaves, which however I was only lately able to pursue, and as they have not been subjected, so far as I can in this remote district ascertain, to a careful chemical examination either in this country or on the continent, the following observations may be interesting. It is, I believe, to Dr. Jeffreys of Liverpool that we are indebted for the first notice in this country of the medicinal virtues of the Matico. Its action, when applied externally as a styptic, has been established in the practice of Dr. Munro of Dundee, and Dr. Lane of Lancaster has also lately communicated the results of his experience regarding its internal administration, which are confirmatory of its South American reputation. On the continent, a short notice of it has appeared in the *Pharmaceutische Central Blatt*, Jan. 1843, by Dr. Martius, and a brief and imperfect account of it is also given in the *Dictionnaire* of Merat and Lenz. It is stated by Dr. Martius, that like the Gunjah, which the East Indian prepares from the *Cannabis Indica*, the leaves and flowers of the Matico have been long employed by the sensual Indians of the interior of Peru to prepare a drink which they administer to produce a state of aphrodisia.

* Communicated by the Chemical Society; having been read April 1, 1844.

The sample of Matico which I received consisted of the leaves of the plant, which were of various sizes, from two to eight inches long, and appeared to have been dried, as they could easily be rubbed to powder between the hands. It is indeed stated by Dr. Martius, that the Indians dry the leaves over a fire, taking care not to allow them to be touched by the flame or burnt. Another sample which I lately procured had the same appearance, but contained, mixed up with the leaves, a considerable quantity of the flowering twigs and woody stems of the plant. The leaves and twigs were compressed together and flattened into a cake.

The botanical history of the Matico appears to be but imperfectly known; and there exists a difference of opinion even as to the class of plants from which it is derived; Dr. Martius, in the *Phar. Central Blatt*, considering it to belong to the *Phlomis* tribe, while in the *Flora Peruviana* it is described as a *Piper*. By immersing a specimen of the plant, which seemed tolerably perfect, in warm water, I was able to examine it with considerable accuracy, and certainly its characters appear very unlike those which belong to the Phlomis family. The stems are woody, round and pubescent ; the leaves sessile, acuminato-lanceolate, rugose and crenate, their upper surface of a dark green, and their lower of a pale green colour. It is said that the name of the plant is derived from a Spanish soldier named Matico, who, lying desperately wounded and bleeding to death, in his agony caught accidentally some of its leaves, and by their application arrested the hæmorrhage and healed the wound. The leaves have a strong aromatic, slightly astringent taste, and the smell and taste of their infusion in water very much resembles that of the tea prepared by the country people in Ireland from the leaves of our indigenous *Salvia verbenaca*.

The following experiments were undertaken to ascertain the pharmaceutical characters of the leaves.

Experiment 1.—Reduced half an ounce of the leaves to a coarse powder and macerated in six fluid ounces of cold distilled water. In ten minutes the liquid had a pale amber colour and the specific gravity 1·003; in thirty minutes its density had increased to 1·004 and its colour had become of a deeper amber; it possessed the peculiar aromatic astringent taste of the leaves. In two hours the liquid had specific gravity 1·005, gave a deep olive precipitate with tinct. ferri muriat., and a yellow white with acetate of lead; tartrate of antimony produced no change. In three hours its colour was rather more brown, specific gravity 1·005. In five hours specific gravity 1·005½; when dropped into a solution of gelatine

it produced no change. In seven hours specific gravity had not increased. In nine hours no change. The maceration having been continued for twelve hours longer, the density was not increased. The filtered liquid had a rich orange brown colour, was neutral to litmus, and possessed the smell and taste of the leaves. The following reagents were applied. Solution of gelatine, tartar-emetic, and perchloride of mercury produced no change.

Tinct. ferri muriat. $\left\{\begin{array}{l}\text{Deep black colouring and dark olive} \\ \quad \text{precipitate.}\end{array}\right.$

Acetate of lead . . $\left.\begin{array}{l} \\ \end{array}\right\}$ Copious canary yellow precipitates.
Infusion of nutgalls

Sulphuric acid . .
Nitric acid . . . $\left.\begin{array}{l} \\ \\ \end{array}\right\}$ Brown-yellow precipitates.
Muriatic acid . .

Caustic potash deepened its colour without throwing down a precipitate. Returned the liquid to the leaves and added 6 oz. cold distilled water, which reduced its specific gravity to 1·003. In three hours its density had not increased; poured off the liquid without expression and macerated the same leaves in 6 oz. cold water. In three hours the liquid had a light yellow colour and specific gravity 1·002; gave a very minute deposit with acetate of lead. Poured off the liquid and macerated for fourteen hours in the same quantity of water; specific gravity 1·000.

Experiment 2.—Infused half an ounce of the sub-pulverized leaves in 6 oz. boiling water. In three hours the liquid had the same taste and colour, and exhibited the same reactions as the cold infusion; specific gravity 1·006.

Experiment 3.—Exhausted 1½ oz. of the leaves by boiling with distilled water over a water-bath and evaporating the filtered decoction to the consistence of an extract. The quantity of extract procured weighed 120 grains. It had a dark brown colour and bitter astringent taste, and possessed very little of the aromatic qualities of the leaves.

Experiment 4.—Introduced 3 oz. of the Matico leaves on a perforated metal support into a tin plate still with about two quarts of water and heated over the fire of a table furnace, adding fresh water occasionally to supply the loss. A milky liquid was given off possessing a strong aromatic camphoric odour, and tasting something like a mixture of oil of rosemary and oil of cubebs. It contained a considerable quantity of heavy oil of a light green colour, which slowly subsided to the bottom of the recipient. By returning the milky liquid to the still and distilling again with 2 oz. of fresh leaves, a liquid

was procured from which about one drachm of this oil sub-
sided.

The Oil of Matico has a light green colour; when recent it
has nearly the consistence of good castor oil, but becomes
thick and crystalline on keeping. Its smell is very powerful
and permanent, and its strong camphoric taste remains a con-
siderable time in the mouth. It dissolves readily in æther
and in alcohol; the solutions are colourless. Sulphuric acid
also dissolves it with the production of a beautiful carmine
colour; water added to this solution destroys its colour and
converts it into a milky liquid. Nitric acid when poured on
the oil becomes of a deep amber colour, and converts it into
a resinous matter. Neither caustic potash nor ammonia had
any action on it. After all the oil had been separated by di-
stillation, a deep rich red-brown liquid remained in the still;
it had a bitter astringent taste, without any of the aromatic
flavour of the leaves; it was neutral to litmus; the leaves were
quite insipid. The filtered liquid acted towards reagents like
the infusion (Exp. 1).

Experiment 5.—Added acetate of lead in excess to the fil-
tered decoction (Exp. 4). A light olive precipitate was thrown
down and separated by the filter, the excess of lead was re-
moved by sulphuretted hydrogen from the filtered liquid,
which after this treatment had a light yellow colour, and pro-
duced, with tinct. ferri muriat., merely a slight olive troubling
Ammonia gave a canary-yellow precipitate, and fresh infusion
of nutgalls a light brown precipitate. Evaporated over the
water-bath, a gelatinous extract of the colour and appearance
of thin glue remained. It tasted like the extract of Matico
(Exp. 3), dissolved slowly in water, but was not acted on by
æther or solution of potash. From its solution in water al-
cohol threw down a gray-coloured precipitate, acetate of lead
ditto; perchloride of iron produced no change; caustic pot-
ash and ammonia rendered the solution orange, and threw
down rich yellow precipitates. Neither muriatic acid, nitric
acid or tartar-emetic produced any change. The gelatinous
extract was dissolved in distilled water, the solution was cau-
tiously evaporated over the water-bath to the consistence of a
syrup, which was placed aside in a cool place, and after some
hours a number of distinct prismatic crystals had formed in it.
These crystals had a brown colour and cooling taste; heated
on a slip of platinum foil, they decrepitated and left a yellowish
white residue, which effervesced with hydrochloric acid. In
their solution in water chloride of platinum threw down a
yellow precipitate. Digested the syrupy extract and crystals
in alcohol of 84° (Gay-Lussac), its brown colour changed to

a clear orange, and the salts collected into a gritty cake. Upon evaporating the filtered alcoholic solution, a yellowish-brown extractive matter, *maticine*, remained. It had a disagreeable smell and exceedingly bitter taste, like aloes, which remained a long time on the palate ; it attracted moisture from the atmosphere, dissolved readily in alcohol and in water, but was not acted on by æther : heated on platinum foil it became brown, swelled up without decrepitation, and left a bulky charcoal. Its solution in water was not affected by chloride of platinum, but gave a yellow precipitate with solution of potash, and also with ammonia : chloride of barium did not alter its colour ; tinct. ferri muriat. rendered it of a deep rich carmine colour, but did not form a precipitate.

Experiment 6.—A tincture was made by macerating half an ounce of the leaves, reduced to a coarse powder, with 4 fluid ounces of alcohol of 84°. In half an hour the liquid had a light green colour ; after macerating for six days the filtered tincture had a fine brownish-green colour, and the specific gravity 0·860. It possessed in perfection the aromatic astringent taste of the leaves ; water made it muddy but occasioned no precipitate ; tinct. ferri muriat. gave a copious brown black, and acetate of lead a greenish yellow precipitate ; solution of potash rendered its colour deep orange, and threw down a deep orange precipitate ; ammonia produced a yellow precipitate.

Experiment 7.—The leaves were incinerated and gave 15 per cent. of a light gray ash, which contained the usual ash constituents. I am at the present time engaged with their quantitative examination, after the excellent method of Hertwig, *Annal d. Chemie und Pharmacie,* xlvi. Bds. 1 Heft.

Experiment 8.—When a portion of the leaves from which all the parts soluble in water had been separated was treated with æther and afterwards with alcohol, *chlorophylle* and a *dark green resin* were procured. The resinous matter was heavier than water, adhered to the fingers with great tenacity, and when heated on platinum foil burned with a strong odour of empyreumatic oil, leaving a bulky charcoal.

From the preceding experiments we may conclude that the leaves of the Matico contain the following constituents :—

      1. Chlorophylle.
      2. A soft dark green resin.
      3. A brown colouring matter.
      4. A yellow colouring matter.
      5. Gum and nitrate of potash.
      6. A bitter principle, maticine.
      7. An aromatic volatile oil.

8. Salts.

9. Lignin.

The leaves of the Matico are easily reduced to a fine powder, which has the colour of powdered senna; when mixed with any thick vehicle, as syrup, &c., it presents an excellent mode of administering them, though, as will be evident, only adapted for extemporaneous prescription, as the essential oil, upon which it is probable much of their medicinal effect depends, would be rapidly dissipated by keeping. The cold infusion (Exp. 1), as it extracts all the active principles contained in the plant, seems the best form for obtaining its medicinal properties. The time required for its preparation need not exceed four hours, as in that time water extracts all that can be taken up by a single maceration, and but little is gained by maceration in fresh water, even though continued for a considerable time, as the liquid rapidly acquires its maximum density.

XXXIV. *On Euclid's Twelfth Axiom. By* X. Y.

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

IN addition to the innumerable attempts which have so often been made to demonstrate the twelfth axiom of Euclid, one of a novel description has been given by Mr. Meikle in Jameson's Journal for April last. For this purpose, he proves, 1st, that triangles, the areas of which are equal, have the sums of their angles equal; 2ndly, that if in one triangle the sum of the angles differed from two right angles, so it would in every triangle, and the difference would always be proportional to the area. Thus far the process is liable to no objection; and if it be granted that there is no triangle which can have the sum of its angles inappreciably small, his third step or inference from the second would be a complete *reductio ad absurdum*, viz. that a triangle of a certain magnitude would have its angles either negative, or some two of them amounting to at least two right angles.

It will now be for mathematicians to decide on the doubtful point just stated; but at any rate the first and second propositions will form, as it were, two new instruments wherewith to perfect the theory of parallel lines. Mr. M. has shown that they are fatal to the celebrated demonstration of Bertrand; in which, besides, it is laid down as self-evident that any angle, however minute, may be multiplied to exceed any given angle. This is nearly the same with what is employed by Mr. Meikle, as noticed above.

X. Y.

XXXV. *On certain New Theorems relative to the Conic Sections. By* John William Stubbs, *Esq., M.A., formerly of Trinity College, Dublin.*

IN the Number of the Philosophical Magazine published in November 1843, the author proposed a new method of deducing theorems relating to the right line and circle from theorems generally known. The object of the present notice is merely to show that all the theorems so deduced indicate new properties of the conic sections.

In order to make this manifest, the author borrows the method used with so much success by M. Chasles in deducing the properties of cones of the second degree and of spherical conics, published in the sixth volume of the Transactions of the Brussels Academy, in which he shows that all properties of the circle, which involve merely angles and positions, lead by the consideration of the cyclic planes to properties of cones having one cyclic plane in common, and of their reciprocal cones with a common focal line, and hence to the sections of such cones by a sphere.

The following theorems are believed by the author to be new, as well as those relating to the circle from which they are deduced; they will serve as examples of the fertility of these methods.

I. If a point be assumed on the circumference of a circle and an angle of constant magnitude turn round it, the chord subtending this angle will envelope a circle. Now assuming as a pole this point, and inverting the figure by producing the radius until the rectangle under the original and whole produced radius is constant (Phil. Mag., Nov. 1843), we derive the property, that if a right line and a point be given in position, and if triangles have their vertical angle constant and always at the given point, and their bases coinciding with the given indefinite straight line, their circumscribing circles constantly touch a fixed circle whose centre is in a straight line perpendicular to the given one, and passing through the fixed point. Now conceive the plane of the figure to be a common section parallel to the cyclic plane of a number of cones having one cyclic plane in common, and we deduce from the above considerations, that if a plane (A) and right line (B) be given, and if through the right line two planes be constantly drawn to form with the given plane (A) a trihedral angle, and if the traces of these planes on another given plane (C), passing through the intersection of A and B, include a given angle, the cone of the second order, of which C is a cyclic plane, circumscribing this trihedral angle, will constantly touch a fixed cone of the second order having the same vertex and

cyclic plane. Now taking the curve the intersection of these cones with a sphere the centre of which is at the common vertex, we derive the following theorem :—Given two great circles and a point on a sphere: if through the given point two arcs of great circles be drawn to cut one of the given arcs, so that the distance between the points of section shall be constant, they will form with the other given arc a triangle; the spherical conic circumscribing this triangle, and having the first arc for one of its cyclic arcs, constantly touches a fixed spherical conic having the same arc as a cyclic arc ; and as we change the length of the constant intercept the pole of its cyclic arc lies in a fixed arc of a great circle. Now taking the corresponding properties in the reciprocal cone or curve : if two points on a sphere and an arc of a great circle be given, and from one of the points arcs be drawn, including a given angle, to meet the given arc ; if the points of intersection be joined with the other given point, and a spherical conic be inscribed in the triangle so formed, having its focus at the first given point, it will constantly touch a fixed conic section with the same focus, and whose directrix, as the given angle changes in magnitude, constantly passes through a given point : by making the radius of the sphere infinite, a similar property of the plane conic sections may be deduced.

In a similar manner the following theorems give as results the annexed properties.

II. A chord of a circle passes through a fixed point, the tangents at its extremities intersect in a right line by inversion (the pole at the given point); a chord of a circle passes through a given point in its segments, circles are described touching the given circle at the extremities of the chord, the locus of the point of intersection is a circle; hence,

" If from any point of a fixed line in the plane of a conic section two tangents be drawn to it, and two conic sections confocal with the given one be described so as to touch each the fixed line and one of the tangents at its point of contact, a tangent drawn to them in common will *envelope* a fourth conic section with the same focus."

III. The angle in the same segment of a circle is constant; taking any point in the circle as pole, and inverting, the following property is derived : Given a line and two points in it, and a fixed point in its plane, two circles passing each through the latter point and one of the other two, and intersecting on the given line, cut at a given angle ; hence,

" If through the vertex of an angle of a fixed triangle any line be drawn to meet the opposite side, and if in the triangles so formed conic sections be inscribed, having a fixed point for

their common focus, lines drawn from this focus to their points
of contact with the variable line drawn as above, contain a
constant angle."

Rowlestown, Swords, Co. Dublin,
        June 27, 1844.

XXXVI. *Observations on Waterspouts.*
By EDWARD RIGBY, *M.D., F.L.S.*

To the Editors of the Philosophical Magazine and Journal.
GENTLEMEN,

DURING my passage from Ostend to London, on the
morning of the 20th of July, at about an hour before
sunrise, I had an opportunity of witnessing a meteorological
phænomenon, viz. a waterspout, which is rarely met with in
these latitudes.   The vessel lay on her usual course (about
W.N.W.), Ostend being about fifteen miles in an opposite
direction, the wind, a slight breeze, being dead ahead.

A dense mass of clouds had been forming in the north-east-
ern portion of the horizon, and from the rapid manner in which
it increased, and from its course not corresponding with that
of the wind as we then had it, I was induced to observe to the
officer on deck that a thunder-storm was approaching.   A
smart flash immediately afterwards confirmed my suspicions;
and as the northern extremity of the dark bank of heavy clouds
gradually lifted from the horizon, and its place was occupied
by a streak of dull red misty light, it was evident that torrents
of rain were pouring down from its middle and southern por-
tions.

When at about the distance of $1\frac{1}{2}$ or 2 miles from us the
mate directed my attention to a funnel-shaped pillar of cloud,
which depended from the mass above apparently to within a
short distance from the surface of the sea, and was peculiarly
conspicuous from crossing the streak of dusky red light to
which I have just alluded.   Its form was much contorted, and
varied in its direction every moment, not so much from the
whole mass moving continuously, as from its seeming suddenly
to disappear at one spot, and instantly to assume a different
form and direction close by.   A dense haze, like smoke, rose
from the surface of the water in an oblique direction towards
the depending portion of the cloud, but the distance was too
great and the deck of the steamer too low to enable me to as-
certain whether the water was in the state of whirling foam at
this part as has usually been described.   After continuing for
about a minute, its connexion with the water below appeared
to cease, and the depending portion of cloud diminished con-

siderably in length.   By this time a second spout had formed
at no great distance from the first, and so instantaneously that
I did not notice its commencement, although having my at-
tention at the moment directed to its immediate vicinity; the
connection of this with the surface of the sea, although visible,
was by no means so distinct as in the first instance.

During this time the wind had been rapidly shifting back-
wards and forwards from one quarter of the vessel to the other;
some nimbus clouds suddenly formed overhead and on the
opposite side, and saluted us with a profuse torrent of rain,
unaccompanied however by gusts or lightning.   The spouts
on the starboard side had now gradually shortened and dis-
appeared, and a very long taper one formed at no great di-
stance from the other side of the vessel.   From its closer vicinity
it seemed to be much higher than the others, indeed so much
so, that at a guess I should say it did not form less than an
angle of 70° with the horizon.   Even its lower extremity re-
mained at a considerable distance from the water, and its rapid
alterations in form and length were very remarkable.   At one
moment it had lost nearly half its length, at the next it had
nearly doubled it, while at the same time its curves and con-
tortions varied every instant.

That these phænomena are the results of violent vortices in
the air, depending probably in great measure on irregular and
conflicting currents, arising from very unequal states of atmo-
spheric electricity, is now, I presume, a pretty well-ascertained
fact.   In slight cases, like the one which I have now described,
the water in a state of finely divided particles, like rain or even
mist, is blown up into the air by the whirlwind, and is thus
made to form a continuous line or column with the depending
portion of cloud, which has been elongated from the mass
above by the same vortex-like movement.

It may be easily imagined that in the more violent forms of
these atmospheric phænomena, which occur in tropical cli-
mates, the surface of the sea may not only be raised in the
form of watery vapour and foam, but that a considerable
portion of water may be carried up bodily to a considerable
height by the force of the whirlwind, and be again precipitated
by its own accumulation and weight.

The concussion of the air produced by the discharge of
heavy artillery in the immediate neighbourhood of these phæ-
nomena can readily be supposed to produce a sudden arrest
in the whirling motion of the air, and to be followed by an
immediate descent of the water which had thus been raised.

23 New Street, Spring Gardens,   I am, &c.,
        July 23, 1844.   EDWARD RIGBY.

XXXVII. *Proceedings of Learned Societies.*

GEOLOGICAL SOCIETY.

[Continued from vol. xxiv. p. 378.]

Nov. 1, 1843. THE following communications were read:—1. "On the Fossil Remains of Star-fishes of the Order *Ophiuridæ*, found in Britain." By Prof. Edward Forbes.

After enumerating the several *Ophiuridæ* recorded as British fossils, the author describes four new species, viz. 1. *Ophioderma tenuibrachiata*, and 2. *Ophiura Murravii*, discovered by Dr. Murray in the lias near Scarborough; 3. *Amphiura Pratti*, discovered by Mr. Pratt in the Oxford clay; and 4. *Ophiura serrata*, communicated by Mr. Tennant, from the chalk. The animals of this order appear to have commenced their existence in the earliest periods of organic life, and to have continued to the present day without any great modifications of form, of family or generic value. They seem at present to be much more numerous than at any former period. None of the fossil species is identical with the existing.

2. "On the Geology of the Maltese Islands." By Lieut. Spratt, R.N., Assistant Surveyor H.M.S. Beacon, F.G.S.

The formations composing these islands are tertiary, and appear, from the author's researches, to belong to one geological epoch. They are all of marine origin, and very regularly deposited in parallel strata, but little inclined from the horizontal. They may be grouped under four divisions:—1. Coral limestone; 2. Yellow sandstone and blue clay; 3. Yellow and white calcareous sandy freestone; and 4. Yellowish white semi-crystalline limestone. Each of these groups is characterized by peculiar fossils, some of which are common to more than one. By a careful examination of the organic remains in each, the author was enabled to detect several extensive faults in both islands. These displacements amount generally to about half the present height of the islands above the sea, viz. about 300 feet, and the direction of the faults is transverse to the line of elevation, or the direction of the islands, that is, N.E. and S.W., the chain of islands running N.W. and S.E. Advantage of the irregularities of surface caused by these faults has been taken in constructing the military defences of the island. The author concludes with a detailed account of the several strata and their subdivisions, describing the distribution of the contained fossils, a collection of which accompanied the paper.

Nov. 15, 1843.—The following papers were read:—

1. "On some Fossil Remains of Anoplotherium and Giraffe, from the Sewalik Hills, in the north of India." By H. Falconer M.D., F.G.S., and Capt. P. T. Cautley, F.G.S.

The Anoplotherium is an undescribed species, differing from those of the Paris basin, and much larger, its size being between that of the horse and the small Sumatran rhinoceros. It is founded on two upper jaws, with the rear molars perfect. It is a true Anoplotherium, as distinguished from *Dichobunes*, &c., but closely allied to the *Chalicotherium Goldfussi*. The discoverers have named it *Anoplotherium Sivalense*. The remains were dug out of a bed of clay in the

tertiary strata of the Sewalik hills, mixed up with bones of Sivatherium, *Camelus Sivalensis*, Antelope, Crocodile, &c. The authors describe two species of giraffe. The first, which they designate *Camelopardalis Sivalensis*, is founded on the third cervical vertebra of an old animal, and they infer it to have been one-third smaller than the existing species. The bone is very perfect, and completely silicified. It measures 8 inches, while the same vertebra of the existing species is $11\frac{1}{2}$ to 12 inches. The bone is more slender in its proportions than the existing one, and exhibits a series of specific differences in addition to the size. The second species they name *Camelopardalis affinis*, provisionally, from its close resemblance to the existing African Giraffe, in form and size of teeth, &c. The species is founded on fragments of the upper and lower jaws and on four detached molar teeth. The dimensions agree to within the tenth of an inch with those of a female head in the Museum of the College of Surgeons. The giraffe bones were found along with those of Anoplotherium, Camel, *Crocodilus biporcatus*, &c., in clay and in sandstone in the Sewalik hills*.

2. Prof. Sedgwick commenced the reading of a paper, in continuation of his memoir, " On the Geology of North Wales," read June 21, 1843†.

Nov. 29, 1843.—Professor Sedgwick concluded his paper.

The author maintains the threefold division of the older rocks. The middle division is now illustrated by more detailed sections, especially through different parts of the Berwyn chain. The first and principal section is from the porphyries of Arrenig across the Lake of Bala, and over the crest of the Berwyns to Llangynog. The whole of this section is placed in a fossiliferous system, and the thickness of the beds actually associated with fossils is several thousand feet. The difference between this result and one stated by Mr. Sharpe, is accounted for, first, by a different computation of the thickness of certain beds about the position of which there is no doubt, and, secondly, by a different interpretation of phænomena, Mr. Sharpe terminating his section abruptly against a supposed fault, while Professor Sedgwick makes a regular ascending section, and places in the highest part of the series certain beds which Mr. Sharpe calls *Cambrian*, and regards as a part of a lower and non-fossiliferous group. In short, Professor Sedgwick extends his section among the fossil groups several miles to the east of the supposed line of fault of Mr. Sharpe. Other sections are described, drawn through the southern part of the Berwyns, which is shown to rest on a great trough formed by the Bala limestone. The author then gives a general and detailed account of the physical structure of the whole Berwyn chain, which measured, on the curved line of the water-shed, is not less than thirty miles long. The whole crest of this chain, with the exception of about five miles, is composed of beds superior to the Bala lime-

* The first announcement of the fossil remains of the Giraffe was made by Capt. Cautley in the Journal of the Asiatic Society of Bengal, vol. vii. p. 658 (15th July, 1838).

[† The official abstract of the paper here referred to, of which that noticed above was the continuation, will be found in our preceding volume, p. 246.]

stone. The author then describes the sections on the east side of the Berwyns, and the section on the Ceiriog and the Dee, which connect the part of the protozoic group, which is the exact equivalent of the Caradoc sandstone, with the Denbigh flagstone, which represents the upper Silurian rocks of Mr. Murchison. After discussing the sections in detail he draws the following conclusions:—
1. The base of the fossiliferous system is unknown, for beds (occasionally alternating with contemporaneous porphyries) of great thickness occasionally present fossil bands with *Asaphus Buchii*, &c. These are below the level of the limestone seen in the Arrenig section. 2. That the limestones near Bala (three of which are on the line of section, and one at a still lower level) contain fossils which point to a lower level than the Caradoc sandstone, and rather conform, especially in the lower beds, to the character of the Llandeilo flagstone. 3. That the higher part of the section on the Ceiriog conforms to the best types of the Caradoc sandstone, and passes into the system of the Denbigh flags. 4. That the lower Silurian beds in Westmoreland do not conform to the above type: they may be compared with the upper part of the Ceiriog section, and perhaps with the *highest* part of the Arrenig section; but they admit of no comparison with the lower and by far the thicker part of the protozoic group of North Wales. Lastly, the author, as in a former paper, divides the upper Silurian rocks of Denbighshire, &c. into three primary divisions or groups. The complicated Llangollen sections he puts entirely in the lowest of the three divisions. He confirms his former views by some new details and general remarks, accompanied by lists of fossils.

Dec. 13, 1843.—The following papers were read:—

1. "Notes respecting the Coal Measures, Limestone, and Gypsiferous Strata of the Island of Cape Breton." By Mr. R. Brown.

The author gives the details of certain sections in the coal-fields of Cape Breton, confirmatory of Mr. Lyell's views of the relative age of the gypsum and other strata in that island *.

2. "On the lower Carboniferous Rocks or Gypsiferous Formation of Nova Scotia." By Mr. J. W. Dawson of Pictou, Nova Scotia.

The coal formation of the eastern part of Nova Scotia consists of a great thickness of sandstones, shales and conglomerates of various reddish and gray colours. The lower part of the series is distinguished by the presence of limestones with marine shells and gypsum. In this paper the author examines the structure and relations of the lower or gypsiferous formation, prefacing it with a notice of the general disposition of the rocks of the carboniferous system in the region extending along the shores of the Gulf of St. Lawrence, from Tatmagouche to Antigonish Harbour. The gypsiferous formation is described as met with at East River, Merigonish, Antigonish, and Shubenacardie. The results of Mr. Dawson's inquiries confirm the views advanced by Mr. Lyell in his papers on the geology of Nova Scotia *.

3. "On Concretions in the Red Crag at Felixstow, Suffolk." By the Rev. Prof. Henslow.

The concretions described are more or less spheroidal, fusiform and cylindrical, many of them amorphous masses of a fine-grained, compact, dark brown ferruginous claystone. Their surfaces are smooth, often polished, and they sometimes include organic remains. Prof. Henslow regards them as of coprolitic origin. Resembling them are certain silicified masses, which prove to be the petro-tympanic bones of extinct Cetacea ; and Prof. Owen has determined that Prof. Henslow's specimens belonged to no less than four distinct species of whales of the genus *Balæna.*

Jan. 3, 1844.—The following papers were read :—

1. " On the occurrence of the genus *Physeter* (or sperm whale) in the Red Crag of Felixstow." By Mr. Charlesworth.

In the collection of Mr. Brown of Stanway, is a remarkable fossil, which Prof. Owen proved to be the tooth of a cachalot, and in the Report of the British Association for 1842 states to have been procured from the diluvium of Essex. Mr. Charlesworth, having examined the specimen in question, considers it a genuine crag fossil from the same deposit with the Cetacean remains, described by Prof. Henslow at a previous meeting, as just noticed.

2. " On a Fossil Forest in the Parkfield Colliery, near Wolverhampton." By Mr. H. Beckett.

The author announces the discovery of a remarkable assemblage of stumps of fossil trees in the Parkfield Colliery, all upright and evidently *in situ.* There are two fossil forests, one above the other. In the upper, Mr. Beckett counted seventy-three trees in about a quarter of an acre, and in the lower they appear to be equally numerous.

3. " On the Remains of fossil dicotyledonous trees in an outcrop of the Bolton coal, at Parkfield Colliery." By W. Ick, Ph.D.

This paper relates to the same locality with the last, and includes numerous details of the state of the fossil forest, its geological relations and accompanying fossils. Dr. Ick describes three distinct beds of coal, each exhibiting on its surface the remains of a forest, all included in an assemblage of strata not more than twelve feet in thickness. He considers the trees to have been mostly coniferous, and concludes that they grew on the spot where they are now found.

4. " On a fossil tree found in the coal-grit, near Darlaston, South Staffordshire." By Mr. J. S. Dawes.

This remarkable fossil, although not entire, is thirty-nine feet in length, and its greatest breadth not more than twenty inches. The wood is coniferous.

5. " On the Trap-rock of Bleadon Hill, in Somersetshire." By the Rev. D. Williams.

In consequence of some remarkable facts disclosed by the railway cutting through the western point of Bleadon Hill, the author's views respecting the origin of trap and other aggregate rocks, advanced in former papers, have undergone a material change. In this paper he details the phænomena which lead him, among other conclusions, to maintain that the lime rocks, in the cases under consideration, have been reduced *in situ* by tranquil fusion, and subsequently converted into the trap which now replaces them. The

extent and variety of the subjects embraced in this memoir do not admit of a short notice.

Jan. 17.—The following papers were read :—

1. " On Fossil Crustaceans from Atherfield, in the Isle of Wight." By Prof. Bell.

The fossils described in this paper were preserved in the lower greensand, and belong to the family of *Astacidæ*, probably to the genus *Astacus*. They are distinct from any known recent or fossil species.

2. " On the occurrence of Phosphorite in Estremadura." By Prof. Daubeny and Capt. Widdrington.

The phosphorite rock, the extent of which had been greatly exaggerated by Spanish writers, is situated at a short distance from Logrosan, a village of Estremadura. It lies in an extensive clay slate formation, and is interstratified with the slate, appearing on the surface for about two miles, presenting a breadth of usually about twenty feet, and a thickness as far as could be ascertained of ten. Its presence does not appear to communicate fertility to the soil. It is composed of phosphate of lime, associated with fluoride of calcium, oxide of iron and silica. The authors examined it with a view to its employment as a manure ; but great difficulties exist with respect to its transportation.

3. " On the Cretacean Strata of New Jersey, and other parts of the United States." By Mr. Lyell.

The author proves, from a careful examination of their fossils, that the ferruginous and greensand formation of New Jersey corresponds to the uppermost part of the cretaceous system in Europe. Four or five, out of sixty fossil shells, are identical with European species, giving an agreement of 7 per cent., whilst a great number of the remainder are nearly allied to and represent species from the middle and upper part of the European cretaceous beds. Teeth of sharks, some of them allied to known cretaceous forms, and vertebræ of Mososaurus and Plesiosaurus accompany them. The upper fossiliferous division of the New Jersey cretacean deposit, observed by Mr. Lyell at Timber Creek, near Philadelphia, judging from the evidence afforded by certain of its fossils, of which, however, the great part, especially of the corals, are new, must be regarded as equivalent to the uppermost (Maestricht) part of the cretaceous system. Among the Echinodermata and Foraminifera are several characteristic cretacean forms.

Jan. 31.—The following papers were read :—

1. " A Vertical Section of the Strata between the Chalk and the Wealden on the South-east Coast of the Isle of Wight." By Mr. Simms.

The thickness of the upper greensand given in this section is 104 feet, that of the gault 146 feet, and that of the lower greensand 754 feet 3 inches ; giving a total thickness of the beds beneath the chalk of 1004 feet 3 inches.

2. " A Report on the British Lower Greensand Fossils in the Society's Collection." By the Curator, Prof. E. Forbes.

There are 131 species of Mollusca, and between 30 and 40 Ra-

diata and Annelida in the Society's cabinets. Of the Mollusca, 60 are additions to the British greensand fauna, mostly discovered within the last twelve months. Half of this number are new species ; and among the rest are many characteristic Neocomian forms.

3. "A Report on the Collections of Fossils from Southern India, presented to the Society by Mr. Kaye and the Rev. Mr. Egerton." By the Curator.

The species from the beds discovered by Mr. Kaye at Pondicherry are, with a few exceptions, new. Among those from Verdachellum and Trinconopoly, are several well-known greensand fossils. The Pondicherry beds appear, from the evidence afforded by their organic contents, to belong to the lowest part of the lower greensand ; whilst those at Verdachellum and Trinconopoly may be referred to the upper greensand. In this Report 156 new species of Mollusca are described and named.

4. "On the European Equivalents of the Permian System, with a General View and Table of its Organic Contents." By Mr. Murchison and M. de Verneuil.

The chief objects of this paper are,—1st, to sustain Mr. Murchison's original opinion, that the rothe-todte-liegende forms the true base of the Permian system ; 2ndly, to point out the equivalents in Western Europe of the Russian series of this age, and to extend the upper palæozoic rocks, so as to embrace the lower part of the bunter sandstein ; 3rdly, to analyse the flora and fauna, showing, that whilst connected downwards with the carboniferous rocks, they were entirely dissimilar from those of the overlying trias ; and, lastly, to vindicate the use of the collective word *Permian*, and its application to a recently published map of England, as derived from a group of strata never previously united through their geological relations and organic remains.

Feb. 21.—The following papers were read :—

1. "Some account of the Strata observed in the course of the Blechingly Tunnel, Surrey, in the year 1841." By Mr. Simms.

The tunnel was carried through a spur of a range of hills, formed by the escarpment of the lower greensand. In the line of the cutting, the spur consisted chiefly of Weald clay, and proved to form part of an anticlinal axis, which extends across the Weald from the chalk of the North Downs in Surrey, between Merstham and Garlstone, to the chalk of the South Downs in Sussex, near Ditchling.

2. "Some Remarks on the White Limestone of Corfu and Vido." By Captain Portlock, R.E.

The author has found fossils in the limestone of Vido. • They are very locally distributed, Ammonites in one place, and Terebratulæ in another ; the former in bad condition, the latter very perfect. They appear to be nearly allied to *Terebratula Pala* and *T. resupinata*, oolitic species, and to a species from Dundry. Captain Portlock regards them as new, and names the species *T. Seatoni* ; inferring, from their presence, that the limestone in question is probably oolitic.

3. "Remarks on *Sternbergia*." By Mr. J. S. Dawes.

The author considers the fossils of this anomalous genus of extinct vegetables as merely casts of the medullary cavities of exogenous

trees, similar to that at Darlaston, lately described (*suprà*, p. 215). The transverse plates which compose the interior of some of these borders, he considers as agreeing with the laminæ of the pith; and the rings on the external surface of others as produced by the same cause. He described specimens in which the so-called *Sternbergia* formed the centre or pith of fossil stems.

4. "On a Fossil Crustacean from New Holland." By Prof. Thomas Bell.

This, the only fossil crustacean as yet found in Australia, was procured by Lieutenant Emery, and forwarded by Mr. W. S. Macleay, who considered it as probably a *Thalassina*. Prof. Bell regards it as a new *Thalassina*, nearly allied to the only known living species of that genus, and names it *T. antiqua*.

March 6.—A paper was read, entitled, "Contributions to the Geology of North Wales." By Mr. Daniel Sharpe.

The observations contained in this memoir were made during a tour through a large part of North Wales, with the object of endeavouring to ascertain what beds lie below the Silurian rocks which have been described by Mr. Murchison, and whether any organic remains are to be found in them. Mr. Sharpe enters into full particulars on the geology of many localities, commencing at Llangollen, in the district examined by Mr. Bowman, and working gradually westward. The details do not admit of a brief abstract. In comparing the Silurian formations of North Wales with those of Shropshire, &c., Mr. Sharpe calls attention to the prevalence of slaty cleavage in the former, to the greater thickness of the Welsh formations, and to the scarcity of organic remains in North Wales in beds which elsewhere are crowded with fossils. He concludes by some observations on slaty cleavage.

A Note, by the Curator, on the fossil species of *Criseis*, discovered by Prof. Sedgwick and Prof. Ansted, was also read at this meeting.

March 20.—The following papers were read :—

1. "Report on the Collection of Fossils from Malta and Gozo, presented by Lieut. Spratt, R.N." By the Curator.

From an examination and comparison of the fossils, it would appear that the Maltese Islands are formed of tertiary strata of the Miocene period, from which there are between eighty and ninety species of organic remains in the Society's collection.

2. "On the Origin of the Gypseous and Saliferous Marls of the New Red Sandstone." By the Rev. David Williams.

The author gives an account of a section through the west end of Worle Hill, near Weston-super-Mare, which he considers throws important light on the origin of the marls in question. He classes them among formations of volcanic origin.

3. Extract of a Letter addressed to the Rev. Dr. Buckland, by Mr. W. C. Trevelyan, "On some remarkable Fractured Pebbles from Auchmithie, near Arbroath."

These pebbles are found in the old red conglomerate, and consist of granite, porphyry, gneiss, jasper and reddish quartz. They are fractured and contorted in a remarkable manner, and present appearances of softenings, and of adhesions subsequent to the fracturing.

April 3.—The following papers were read :—

1. " On the Traces of the Action of Glaciers at Porth Treiddyn, in Caernarvonshire." By Mr. R. W. Byres.

The author notices a number of evidences of glacial action, instances of rounded, polished, furrowed and striated rocks in the neighbourhood of Tremadoc, similar to those observed in several localities around Snowdon by the Rev. Dr. Buckland.

2. " On the occurrence of Fossils in the Boulder Clay." By Mr. R. Harkness.

Fossils are rare in the boulder clay, and are only found in the thicker parts of it. Such portions the author considers as having been deposited in a deep sea, the lower parts of which had a temperature sufficiently warm for the support of organized beings. In the thicker beds the boulders are rounded and polished. Such parts of the clay as are free from fossils, and contain angular boulders, Mr. Harkness considers to indicate the former existence of a shallow sea, with a temperature so cold as to prevent the existence of animal life. Fossiliferous localities occur in the boulder clay of the southwestern parts of Lancashire, where the marl abounds with remains of shells, mostly in a fragmentary state.

3. A letter was read from Dr. Owen Rees on the question of the existence of Fluoric Acid in recent Bones, which the experiments of Dr. Rees would go to disprove. The bones examined by him were tested both before and after calcination, but in no case could he detect the least trace of fluoric acid in recent human bone. In fossil bones it exists in large proportions*.

April 17.—The following papers were read :—

1. "Observations on the Geology of the Southern Part of the Gulf of Smyrna and the Promontory of Karabournoo." By Lieut. T. Spratt, R.N.

The author takes up the geology of the neighbourhood of Smyrna at the point where the observations of Messrs. Strickland and Hamilton terminate †, and gives a detailed account of the schists and limestones of Mount Corax and Cape Karabournoo, and of the extensive freshwater tertiary formation which borders them on the sea-coast, and is continued into several of the neighbouring islands. He notices the presence of igneous rocks of two distinct ages, viz. serpentine, older than the tertiary, and trap, which had been erupted after the deposition of the tertiary, greatly disturbing, and in places overflowing it. His observations prove the existence at a former period of a great freshwater lake in the eastern part of the Archipelago, where now there is a deep sea.

2. "Note on the Fossils found in the Tertiary Formations described in the preceding paper." By the Curator.

An examination of the fossils found in the freshwater beds, described by Lieut. Spratt, shows that formation to have been deposited during the Eocene period.

3. " On the Remains of Fishes found by Mr. Kaye and Mr. Cunliffe in the Pondicherry Beds." By Sir Philip Grey Egerton, Bart., M.P.

In this paper the author describes fourteen species, mostly new,

[* See p. 222, *suprà.*]   [† See Phil. Mag. S. 3. vol. xi. p. 202.]

twelve of which belong to the placoid order, one is a ganoid, and one a cycloid fish. Among them is *Corax pristodontus*, identical with the Maestricht species. From the evidence afforded by the examination of these fishes, Sir Philip Egerton takes the same view of the age of the beds which was inferred by Prof. E. Forbes from the invertebrate remains (p. 217), considering them as belonging to the cretaceous æra, though he is inclined to place them higher in the series.

4. " On the occurrence of a Bed of Septaria containing Freshwater Shells, in the series of the Plastic Clay at New Cross, Kent." By H. Warburton, Esq., M.P., F.R.S., Pres. G.S.

The bed described does not form a continuous stratum, but occurs with interruptions and intervals in the condition of Septaria, which contain remains of shells of the genera *Paludina* and *Unio*. A section of the locality, showing the position of the bed, was made by Mr. Simms. In France similar associations of freshwater remains have been noticed in the plastic clay series by M. D'Archiac.

May 1.—The following papers were read :—

1. " Report on the Fossils from S. Fé de Bogota, in South America, presented to the Society by Mr. E. Hopkins." By the Curator.

These fossils belong to seventeen species of Mollusca, of which nine are identical with species from the same locality, described by Von Buch, D'Orbigny, and Lea. They are from a dark compact limestone, which the reporter regards as a member of the lower part of the cretaceous system. Eight of the species are new.

2. " Comparative Remarks on the Sections of the Strata below the Chalk, on the Coast near Hythe in Kent, and Atherfield, in the Isle of Wight." By Dr. Fitton.

In this paper the author enters into an elaborate review of the state of our knowledge of the lower greensand in England, and compares the several deposits and their fossil contents. After commenting on the relations of that formation at Hythe, with the several strata of the Atherfield section, and showing the comparative state of our knowledge of them when he read his memoir on the sub-cretaceous strata in 1824, with the state of the subject at the present day, he proceeds to examine the researches of M. Montmoulin in Switzerland, M. Dubois de Montperreaux in the Caucasus, MM. Leymerie and D'Orbigny in France, and M. Roemer in Germany, and to prove the identity of the beds styled by some of those authors Neocomian with his lower greensand. Regarding both names as objectionable, the former on account of its being derived from a locality which will not serve as a type for the formation, and the latter because it implies erroneous relations, he proposes the term " *Vectine*," from the island in which we find the strata of this part of the cretaceous system best displayed.

3. A letter from Mr. Simms was read, noticing the occurrence of Lower Greensand clays resting on the Wealden at the cutting near Jeston turnpike, on the Maidstone line of railway.

4. " On the Section of the Lower Greensand, between Black Gang Chine and Atherfield Point." By Capt. Boscawen Ibbetson and Prof. E. Forbes.

After describing sixty-three distinct strata, presenting a total

thickness of 843 feet, which constitute the lower greensand in this section, the results of a chemical examination of the several strata were stated. The strata were grouped under three divisions, the lowest consisting of fossiliferous clays, the middle of Gryphæa sands, and the upper of more or less ferruginous sands, mostly free from fossils. An inquiry was entered into respecting the conditions under which these beds were deposited, and the state of animal life in the cretaceous seas during their formation. The distribution of their fossils was given in detail, and the results of the inquiry stated as proving the unity of the lower greensand, considered as a member of the cretaceous series. Capt. Ibbetson laid before the Society a model of the section, on the scale of three feet to the mile, constructed by himself from trigonometrical survey, on which the several strata described in the paper were laid down minutely.

5. " Description of the mouth of a *Hybodus*, found by Capt. Ibbetson in the Isle of Wight." By Sir Philip Grey Egerton, Bart., M.P.

This fish was found at the junction of the Wealden with the lower greensand. The specimen sets at rest the question of the relative characters of the upper and lower teeth, and the general contour of the individuals composing the genus *Hybodus*, confirming the views of Prof. Agassiz. The species is new, and Sir Philip Egerton proposes to name it *Hybodus Bassanus*.

6. The President read extracts from letters lately addressed by M. Dubois de Montperreaux and by Prof. Agassiz to Capt. Ibbetson, on the subject of the Neocomian. The former of these geologists states that the Neuchatel beds cannot be regarded as complete, or as the type of that formation, which he considers is best developed in the Crimea and Caucasus. The latter considers the Neocomian as a peculiar stage, and that the very lowest of the cretaceous system*.

May 15.—The following papers were read :—

1. A letter from Dr. Ick on some new fossil Crustacea, from the South Staffordshire coal-field.

2. " On the Geology of Cape Breton." By Mr. R. Brown.

The newest stratified rocks in the island of Cape Breton belong to the coal formation. The coal-field of Sidney occupies an area of 250 square miles, and appears, from the dip of the beds, to be a portion of a still more extensive field. The coal measures repose on millstone grit of variable thickness and great extent. Beneath the millstone grit lies carboniferous limestone, associated with extensive beds of gypsum and marls. These gypsiferous beds lie upon conglomerates, which pass downwards into slates, corresponding to the grauwacke formation in Europe. In places the eruption of red granite has converted the schists into white marble. Igneous rocks of various forms, granites, porphyries, greenstone and trap, occupy a considerable portion of the island†.

3. " On the anthracite formation of Massachusetts." By Mr. Lyell.

The author states that the fossil plants associated with the anthra-

[* On the subjects of these papers on the lower greensand and Neocomian, see Phil. Mag. S. 3. vol. xxiv. p. 222, 224, 308.]

[† See p. 214, *suprà*; also vol. xxiv. p. 146, 149.]

cite of Wrentham, Cumberland and Mansfield on the borders of the States of Rhode Island and Massachusetts, are of true carboniferous species. The strata containing them, as shown by Hitchcock, Jackson and others, pass into mica-schist, clay-slate, and other metamorphic rocks. The bed of plumbago and anthracite, two feet thick, at Worcester, Massachusetts, is separated from the anthracite before mentioned, by a district of gneiss, thirty-five miles wide. This bed Mr. Lyell regards as coal in a still more completely metamorphic state, all the volatile ingredients having been discharged and carbon alone remaining, the accompanying coal-shales and grits having been turned into carbonaceous clay-slate, mica-schist, with granite and quartzite. No similar beds are found in the North American Silurian formations.

May 29.—The Rev. Professor Sedgwick read the conclusion of his " Memoirs on the Geology of North Wales."

June 12.—The following papers were read :—

1. " On Fluorine in Bones, its Source and its Application to the ascertainment of Geological Time." By Mr. J. Middleton.

The author having analysed and determined the amount of fluoride of calcium in recent bone, in that of an ancient Greek, of a mummy, and in the bones of fossil vertebrata from the Siwalic hills, found the proportions increase according to the age. He instituted a series of experiments on aqueous deposits of different kinds and ages, and found fluorine also present in them, with a single exception. He refers its presence in bones to deposition from fluids, and hence accounts for its great abundance in fossil bones, which had long been exposed to aqueous infiltration*.

2. " On the Cliffs of Northern Drift on the Coast of Norfolk between Weybourne and Happisburgh." By Mr. J. Trimmer.

The author describes such changes as have occurred along the line of cliffs between Weybourne and Happisburgh since Mr. Lyell's visit in 1840†. He gives an account of the present state of the pinnacle of chalk at Old Hythe point, and holds with Mr. Lyell, that it is separated from the fundamental chalk by the ferruginous breccia of the crag. The southern mass of chalk near Trimmingham has been greatly reduced. The author's observations lead him to conclude that the till and freshwater deposit between Mundesley and Trimmingham are so interlaced as to indicate that they were in part contemporaneous. He regards the northern drift containing shells as having been transported on ice, but as differing materially from ordinary raised beaches. Mr. Trimmer concludes from the phænomena exhibited at Happisburgh, that the land on which the elephant and hippopotamus lived was submerged beneath an icy sea, and that there was an antecedent conversion of a sea-bottom, the Norwich crag, into a terrestrial surface.

3. A letter was read from Mr. Jeffreys of Swansea, to the Rev. Dr. Buckland, describing several raised sea-bottoms, forming platforms on the shores of Loch Carron and the neighbouring coast of Scot-

[* On the subject of this paper see p. 219, *suprà* ; also p. 14, 122.]
[† Mr. Lyell's paper was published in Phil. Mag. S. 3. vol. xvi. p. 345.]

land, some of them fifty feet and more above high-water mark, containing shells similar to those found living in the neighbouring sea.

June 26.—The following papers were read :—

1. " Notice of the Tertiary Deposits in the South of Spain." By Mr. Smith of Jordan Hill.

The author has found a tertiary deposit bordering the Bay of Gibraltar. This agrees in its fossils with those observed by Colonel Silvertop in Murcia and Granada. Mr. Smith has found similar beds at Cadiz, and between Xeres and Seville. All these deposits agree with those of Malta and Lisbon, and belong to a great expanse of miocene tertiary, which runs from Greece to the Straits of Gibraltar, and the shores of Portugal, and from Malta to Vienna.

2. " On the Stonesfield Slate of the Cotteswold Hills." By Mr. Buckman and the Rev. P. B. Brodie.

The Stonesfield slate in the Cotteswold range occupies an area of more than fifty miles. It is identical in lithological and palæontological characters with that at Stonesfield. It is so intermixed with as scarcely to be separable from the ragstone, and hence the authors conclude that it is a part of the great oolitic formation, and was deposited by the same sea in which the great oolite itself was formed, and owed its origin to certain mixed conditions arising from the influx of rivers into an ocean interspersed with numerous scattered islands, abounding with a luxuriant vegetation, and inhabited by numerous terrestrial animals ; which view, they hold, is borne out by the quantity of plants which occur throughout the Stonesfield slate beds, and also from the relics of land animals, such as the Didelphis and Pterodactylus. The clays which lie upon the slate may possibly represent the Bradford clay, or if not, are the equivalents of certain clay beds, containing Apiocrinites, which in Wiltshire separate the firestone from a lower stratum of freestone of a coarser texture.

3. " Description of a Fossil Ray from Mount Lebanon." By Sir Philip Grey Egerton, Bart., M.P.

The author describes a new and most remarkable fossil fish brought from Syria by Capt. Graves, R.N. It is a true ray, much resembling those of the present period, but entirely surrounded by a broad flexible cartilagino-membranous fin. The skin appears to have been smooth, and there are no traces of dermal spines, tubercles, or defensive weapons. From its apparent helplessness, Sir Philip Egerton conjectures that it was probably armed like the torpedo, to which it is in some respects allied, with an electrical apparatus. He names it *Cyclobatis oligodactylus*.

4. " Description of some New Species of Fossil Fish, from the Oxford Clay of Christian-Malford." By Sir Philip Grey Egerton, Bart., M.P.

Three new species are described in this communication, the *Lepidotus macrochirus*, the *Leptolepis macrophthalmus*, and the *Aspidorhynchus enodus*. They were procured by the Marquis of Northampton and Mr. Pratt.

5. " On certain Calcareo-corneous Bodies found in the Outer Chambers of Ammonites." By Mr. H. E. Strickland.

These bodies are semicircular, very thin, slightly concave plates, usually corneous, sometimes more or less calcareous. Mr. Strickland regards them as having formed laminar appendages to the animals of the Ammonites, adapted to discharge some unascertained function. They resemble the two expanded valves of *Aptychus*, soldered together; and the author considers them as allied to that fossil, to which he attributes a similar origin.

ROYAL ASTRONOMICAL SOCIETY.

[Continued from p. 73.]

March 8, 1844 (*continued*).—II. Elements of the Comet of Faye. By Professor Henderson.

From observations made at Cambridge on December 16 and January 15, and at Greenwich on February 8, the following elements of the comet's orbit have been obtained, which are likely to be more correct than those formerly given :—

Epoch of mean anomaly for December 31, 1843, mean noon at Greenwich .... $\left.\right\}$ 11 32 14
Longitude of perihelion...................................   43 39 46
Eccentricity ....sin.   34 0 38
Log. of semi-axis major......... 0·56533
Mean daily motion .... 503″·52
Time of revolution .... 7·0468 sidereal years.
Longitude of ascending node............................ 211 59 12
Inclination 11 28 48

Motion direct.

Ephemeris for 8ʰ *Mean Greenwich Time.*

| Day. | Right ascension. | Declination. | Distance from | |
			Sun.	Earth.
1844.	h m s	° ′		
Feb. 8	5 23 36	+6 18	2·008	1·325
12	5 27 32	6 45	2·029	1·380
16	5 31 36	7 11	2·051	1·437
20	5 35 52	7 37	2·073	1·495
24	5 40 20	8 2	2·096	1·555
28	5 45 4	+8 27	2·118	1·616

Edinburgh, Feb. 14, 1844.   T. HENDERSON.

III. Elements of the Comet of Faye. By J. R. Hind, Esq. Communicated by the Rev. R. Main. Inserted in the Monthly Notices, vol. vi. No. 5.

IV. Letter from Professor Encke (translated), dated Berlin, Feb. 19, 1844. Communicated by G. B. Airy, Esq., Astronomer-Royal.

"The comparison of the Berlin observations of the Comet of Pons, made in the year 1842, with the elements which were derived from the observations up to the year 1838 and from the mass of Mercury thence deduced, and with the total disturbances up to 1842, has shown that the remaining errors are quite insignificant; and I find, in fact, for the less accurate observations at the commencement, when the comet was very faint,—

		$\Delta\alpha.$	$\Delta\delta.$
Feb.	9	$- \overset{''}{1}\cdot7$	$+ \overset{''}{18}\cdot2$
	11	$+ 17\cdot9$	$- 1\cdot3$
	12	$+ 16\cdot9$	$+ 4\cdot7$
March	3	$- 1\cdot6$	$- 4\cdot6$

And for the latter and more accurate observations,—

March	11	$+ \overset{''}{4}\cdot8$	$+ \overset{''}{3}\cdot6$
	20	$+ 3\cdot7$	$- 5\cdot3$
	23	$+ 9\cdot3$	$- 8\cdot3$
	24	$+ 0\cdot5$	$+ 2\cdot2$
April	6	$+ 7\cdot5$	$- 4\cdot2$
	7	$+ 1\cdot0$	$+ 3\cdot4$

both sets of errors in right ascension and in declination being expressed in seconds of space. Now that I am preparing the Ephemeris for 1845, it would be highly important for me to compare the whole of the observations of 1842, especially those of the Cape of Good Hope. Up to the present time I have received no communication concerning them. Should the results of these observations be known to you, I should be much obliged if you would do me the favour to communicate them.

"The present remarkable comet we have seen up to the 13th of February, but we shall probably be able to follow it still further.

"The last observations were,—

	R.A.	Declination.
	h m s	° ′ ″	° ′ ″
Feb. 7	7 15 34	80 39 57·8	+ 6 10 51·9
10	10 59 8	81 22 21·5	6 31 54·9
13	7 40 6	82 3 54·6	+ 6 51 42·8

V. Occultations of Fixed Stars by the Moon, observed at Hamburg. By C. Rumker, Esq. Communicated by Dr. Lee. See Monthly Notices, vol. vi. No. 5.

VI. Further Remarks on the Revision of the Southern Constellations. By Sir J. F. W. Herschel, Bart.*

The idea originally proposed of entirely remodelling the southern constellations, has (after very mature consideration and much discussion, and after consulting the opinions of some of the most eminent continental astronomers, which have been found very adverse to the idea of so decided a change) been laid aside; at least in so far as regards the new catalogue of stars, now printing under the authority of the British Association. It is conceived, however, that if the nomenclature of the constellations, generally, be ever destined to undergo a systematic change at all (and many reasons may be adduced for considering such a change desirable), the first and most important step towards it will be found in the work above alluded to, and in the catalogues now publishing simultaneously with it on the same system of nomenclature, which clear the ground of all existing confusion; and, by assembling into one distinct view, and under names and numbers at least definite and recognised, all the

[* An abstract of Sir John Herschel's former paper on this subject will be found in Phil. Mag. S. 3. vol. xix. p. 582.]

individuals of which the new groups must be composed, it will be easy at any future time to pass, by a single table of synonyms and by one decided step, from one to the other system, whenever the convenience and consent of astronomers may dictate the propriety of a change.   Such views, if entertained, would render the nomenclature of the present catalogues so far provisional that a more rational and convenient system of groups (confined not to the southern hemisphere, but extending over both) may yet be contemplated by astronomers.   Nevertheless, so long as the ancient system is at all retained, a general and scrupulous adherence to the nomenclature here adopted is most earnestly recommended to the astronomical world, as the only mode of escape from that state of confusion at present quite intolerable.   As regards the southern constellations, the following are the principles proposed ; viz.

1. That all the constellations adopted by Lacaille be retained, and his arrangement of the stars preserved ; subject, however, to certain alterations hereafter specified.

2. That all the stars, having a doubtful location, such as those which Lacaille (after the manner of Ptolemy) has considered as ἀμόρφωτοι (unformed), be included within the boundaries of either one or other of the contiguous constellations, so as to preserve a regularity of outline and nomenclature.

3. That all the rest of Lacaille's stars be placed within the boundaries laid down by him, with the following exceptions :—First, a few stars which are located too far from the border of the constellations in which they are registered, to admit of an uniform contour of the lines.   Secondly, such stars as have been previously observed by Ptolemy or Flamsteed, and by them located in other constellations, or which interlace and are confusedly mixed with such previously observed stars *.   Thirdly, the four stars that are placed by Lacaille in the end of the Spear of Indus, but which are now assumed to form part of the constellation Pavo, in order to render the contour of these two constellations less circuitous.

4. That the letters, selected by Lacaille, be adopted in preference to those introduced by Bayer in Argo, Centaurus, Ara and Lupus. That the Greek letters (with a few exceptions) be retained only as far as stars of the fifth magnitude inclusive.   That no Roman letters be at present used, except in the subdivisions of Argo, subsequently mentioned.

5. That Argo be divided into four separate constellations, as partly contemplated by Lacaille ; retaining his designations of Carina,

* A single exception to this rule occurs in the case of the last star in the constellation Piscis Australis, in Ptolemy's catalogue, which Bayer has denoted by the letter *x*, and which is presumed to be the same as that which has been designated by Lacaille as γ Gruis.   As there is some ambiguity, however, in the position of this star in Bayer's map, it is assumed (like some other stars already mentioned) as common to both constellations, in order to adjust this discordance ; and, in the catalogue above-mentioned, Lacaille's designation of γ Gruis is retained, on account of its forming the principal object in the head of that constellation.

Puppis and Vela; and substituting the term Malus for Pixis Nautica, since it contains four of Ptolemy's stars that are placed by him in the mast of the ship.

6. That the original constellation Argo, on account of its great magnitude and the subdivisions here proposed, be carefully revised in respect of lettering, in the following manner:—First, in order to preserve the present nomenclature of the principal stars, all the stars in Argo (that is, in the general constellation, regarded as including the subdivisions above-mentioned) indicated by Greek letters, by Lacaille, to be retained, with their present lettering, under the general name Argo. Secondly, all the remaining stars, to be designated by that portion of the ship in which they occur, such as Carina, Puppis, Vela and Malus, and to be indicated by the Roman letters adopted by Lacaille, as far as the fifth magnitude inclusive. And no two stars, far distant from each other in the same subdivision, to be indicated by the same letter; but, in cases of conflict, the greater magnitude is to be preferred; and, when they are equal, the preceding star to be fixed upon.

7. That the constellations, which Lacaille has designated by *two* words, be expressed by only *one* of such words. Thus, it is proposed that the several constellations indicated by Lacaille as Apparatus Sculptoris, Mons Mensæ, Cœlum Scalptorium, Equuleus Pictorius, Piscis Volans, and Antlia Pneumatica, be called by the respective titles of Sculptor, Mensa, Cœlum, Pictor, Volans and Antlia; contractions which have on some occasions been partially used by Lacaille himself, and are very convenient in a registry of stars.

VII. Extract (translated) from a Letter of Professor Bessel to Sir J. F. W. Herschel, Bart., dated Königsberg, January 22, 1844. Communicated by Sir John Herschel.

"I think it may be interesting to you, or to one or other of your astronomical friends who occupy themselves with meridional instruments, to be made acquainted with a result which I have obtained from a theoretical investigation, the object of which was to determine the effect of gravity upon the figure, and consequently on the divisions, of a circle fixed in the vertical plane. The effect will manifestly be, that the radii in the upper part of the circle will be shortened; those in the lower part lengthened; and all, with the exception of the vertical ones, bent downwards. These changes, the magnitude and law of which must depend on the special construction of each circle, in two instances known to me (namely, those of two meridian instruments by Repsold, each of which is furnished with two circles at opposite ends of the axis, each being read by four microscopes), have become prominently sensible; giving rise to this effect, namely, that on turning the instrument through 180°, a different measure is given by its two circles. From this result it follows that the supposition of the amount of change being insensible, or of the influence of gravity on the circle being eliminated by the four readings, is without foundation; and that there is, therefore, cause for apprehending in general that every circle gives an erroneous measure of the zenith-distances, and erroneous to an extent

which, in respect of the existing means of pointing its telescope in a given direction, and reading the divisions, is by no means insensible.

"Now, as it is necessary to be able to determine this influence to such a degree of approximation as to be in a situation to judge whether it is possible to adopt any mode of using the instrument by which the results shall be freed from it, I have been led to undertake the solution of the following statical problem :—' To determine the figure of equilibrium of a circle placed in the vertical plane.' This problem is manifestly considerably complicated. For a circle having m radii, and each pair connected together, not only by the circular rim, but also by a direct connexion, there is (in respect of both circles) an aggregate of $4\,m$ elastic lines, both expansible and flexible, for the determination of which $3 \times 4\,m = 12\,m$ constants are necessary; besides which, $6\,m$ unknown quantities also come into consideration; namely the coordinates of each of the particular points in which two or more of the lines are united, and the directions of the lines with respect to the same; consequently, three unknown quantities for each of the $2\,m$ particular points. The problem, therefore, includes $18\,m$ unknown quantities; and, in the case of Repsold's circles, which have 10 radii, 180. The equations necessary for their determination will be obtained from the condition that the sum of the forces which act not only upon the particular points, but also upon every point of a line, arising from gravity and the connexion of the different parts, shall be in equilibrium.

"The general solution of this problem, which is limited neither by the assumption of a symmetry of figure, mass, or elasticity, nor by that of the absence of primitive tension between the different parts of the whole, may be reduced, as I find, to the solution of $3\,m$ linear equations, and, on the supposition of symmetry, to three such equations. But this last supposition very probably corresponds to no actual case. It is also probable that in no case can we obtain a knowledge of the deviations from actual symmetry, which it is necessary to possess, in order to obtain a result which shall give the required influence in numbers. I find the following general proposition respecting the law of this influence, which is restricted by no particular supposition :—

"If the angle between the initial radius of the circle and the vertical be denoted by u, the angle between the same radius and a point of the division by v, then the variation of this angle arising from gravity, in the vertical plane of the circle, is expressed by $fv \cdot \cos u + f'v \cdot \sin u$, where fv, $f'v$ are functions of v, independent of the position of the initial radius, and dependent only on the construction of the circle.

"This proposition is of considerable importance for practical astronomy. We readily infer from it, that the influence of gravity upon a zenith distance entirely vanishes when the latter is determined by the mean of four observations, namely, observations of the object itself and of its image reflected from a horizontal mirror, repeated in reversed positions of the axis of the instrument. From the same proposition we also easily deduce the mode of arranging the

observations through which the errors of division must be determined, in order that their results may be also independent of the influence of gravity.

" It follows also from this that the astronomer who uses a circle is in possession of the means of rendering his results entirely independent of the influence of gravity ; and that results obtained by different instruments, when this method is followed, must accurately agree. It is likewise to be remarked, that the power of reversing the axis is an advantage with which the independence of the zenith distances of the influence of gravity is connected (and without which such independence could not be obtained).

" Although of no use in a practical point of view, it may not be uninteresting to investigate the numerical result which the theory now developed gives in a particular case, on the supposition of symmetry. For this purpose, I took the case which corresponds to the dimensions of my 3-feet circle by Repsold, and found that one of its radii, the direction of which corresponds to a zenith distance z, alters in length about $-0''\cdot2274 \cos z$: its extremity deviates from its direction at the initial point about $+ 0''\cdot6726 \sin z$; and its direction at the extremity of the arc from that at the initial point by $-0''\cdot5124 \sin z$. Although the supposition of symmetry upon which these results depend is probably only correct in respect of the exterior figure of certain parts of the whole instrument, this calculation may give an idea of the extent to which the influence of gravity will attain ; an extent which, from the complicated nature of the problem, we should not be able, without a calculation, to estimate.

" You may easily suppose that the conclusion I have just obtained, namely, that the attainment of any required degree of precision in results depends only on the observer, appears to me to be the strongest motive for the zealous prosecution of observations. In the ever-memorable days I passed with you at Collingwood, you expressed the opinion, that the prospect of completely attaining the object of any undertaking is of itself sufficient to render it in a high degree attractive."

VIII. A Letter from J. R. Crowe, Esq., British Consul-General of Norway, concerning a Literary Society established at Alten, near Hammerfest. Communicated (with a letter) by Dr. Lee.

Mr. Crowe visited England in the summer of 1843, and gave information concerning the existence of a Society consisting of Swedes, Englishmen and Germans, at Alten, near Hammerfest in Finmark, under the patronage of a Swedish clergyman, the pastor of that district. This Society was in possession of some instruments which had been left there by some French gentlemen of science who were sent to Lapland by Louis Philippe a few years ago. Regular observations of the barometer and thermometer had been instituted according to the plan suggested by Sir John Herschel, and Mr. Crowe was of opinion that if an observatory could be established the Society would cheerfully undertake the working of it.

Dr. Lee munificently furnished the Society with an achromatic transit instrument of 30 inches focal length, and an aperture of 2

inches, on an iron stand, and with a circle 6 inches in diameter, reading to 1′, and with a collection of books on various departments of science : an astronomical clock was also ordered of Mr. Dent, but it was not ready to be sent by the same vessel which conveyed the instruments.   Owing to the exertions of Dr. Lee, other fellows of this Society were induced to contribute books, and the nucleus of a good library was thus formed.   Mr. Crowe took with him from England two minimum thermometers with the intention of placing one on the top of Storvandsfield, the highest mountain in the neighbourhood of Alten, and the other on the highest point of the southern extremity of Spitzbergen ; but he arrived too late to carry the latter part of his plan into effect.   The difficulties of the ascent of Storvandsfield are thus described by Mr. Crowe :—

"The ascent of Storvandsfield was very difficult ; so much fresh snow had fallen as to impede even the snow-shoes, which the party were obliged to use.   The task, however, was accomplished, and the thermometer safely fixed on the highest point.   It was just in time, as one of those sudden gales of wind sprang up, peculiar to high mountain regions, driving and whirling the snow before.   For hours the party were exposed to considerable danger, and by the time it did lull, Mr. Grieve, who had volunteered to superintend the task, was so knocked up as to be unable to proceed, and the guides had to carry him ; fortunately, a shelter of loose stones, erected by the nomadian Laplanders, was reached, where they deposited him, while one of the guides descended for further assistance.   Happily, the cold was not intense, so that a warm bed and rest perfectly recovered him."

Alten is in north latitude 69° 38′, and in longitude 23° 43′ east, and thus by its geographical situation, highly important for certain classes of observations, being the most northern place in the world at which an observatory is established.   The following observations have been uninterruptedly carried on, viz.—

Of the barometer ; the thermometer (with the maximum and minimum temperatures) ; the pluviometer* ; the galvanometer ; declination magnetometer ; land winds and approximate force ; of clouds, their direction, approximate velocity, and general description ; and of the aurora borealis.

XXXVIII. *Intelligence and Miscellaneous Articles.*

To Richard Phillips, Esq.

Dear Sir,

I HAVE received from my brother, at Boston, U. S., some mineralogical notices, recently read before the Boston Natural History

---

[* A Report by Major Sabine and Lieut.-Col. Sykes on Mr. Thomas's Meteorological Observations made at Alten, from 1837 to 1839, will be found in Phil. Mag. S. 3. vol. xvii. p. 295.   In this report the latitude of Alten is stated to be 69° 58′ 3″ N., being 20′ 3″ higher than as given above. Edit.]

Society, which I inclose for insertion in the Philosophical Magazine, if you think they would prove of sufficient interest to your readers.

2 Park Terrace, Highbury,   I am, dear Sir, yours truly,
  August 17, 1844.   E. F. Teschemacher.

MINERALOGICAL NOTICES. BY J. E. TESCHEMACHER, ESQ.

Glauberite, brought from Tarapaca, Peru, by Mr. John H. Blake.

This mineral is found in small crystals, which are imbedded in a beautiful, silky, fibrous borate of lime, published in Alger's edition of Phillip's Mineralogy under the name of *Hayesine*.

This glauberite was analysed by A. A. Hayes, and found to contain

Sulphuric acid....	57·220
Soda	21·324
Lime	20·680
Iron...........	·444

It is essentially a compound of sulphate of lime and sulphate of soda. The first crystals I measured, the figure and measurements of which are in the last edition of Dana's Mineralogy, were extremely dull, so that I could only consider them as approximative. Mr. Hayes afterwards sent me some extremely brilliant crystals, the measurements of which coincided exactly with those of the glauberite from Villa Rubia in Spain. The figure above named is that of the first crystals I found. Subsequently others came to light, of very different forms, produced by other modifications.

It is stated in Dana, that if crystals of glauberite are left in water, they will at length fall to pieces. This is probably true. Those from Tarapaca, if treated carefully with cold water, will permit nearly all the sulphate of soda to be dissolved out, leaving the form of the crystal unaltered, and the edges as sharp as ever. Their appearance before the microscope, after the loss of the sulphate of soda, is that of snowy-white, fine granular sulphate of lime (alabaster). From 15 grains of this glauberite I have obtained $6\frac{1}{2}$ grains anhydrous sulphate of soda, and do not doubt that the whole quantity might with care be extracted. If some forms of these alabaster crystals had been first found and analysed, they would have been pronounced to be pseudomorphous forms of an unknown mineral, of which sulphate of lime had taken the place.

Now, if the action of the solvent powers of various chemical solutions, as well as those of the gases which can be held by water, be studied with care, the natural decomposition of many minerals, as well as their replacement by other substances, in what are called pseudomorphous forms, will be very much illustrated.

Pyrrhite.

Prof. J. W. Webster, of Harvard University, has lately returned from the Azores, and brought with him some interesting minerals, several of which he has most kindly placed in my hands; amongst others, a beautiful arragonite, at first sight much resembling the needlestone.

He also favoured me with two small specimens of a felspathic

mineral, probably albite, on which were several extremely minute but beautiful octahedral crystals, of a deep orange to a wine-yellow colour, the smallest transparent, the largest translucent on edges.

At first I considered this a new mineral, until, on comparing it with the account of pyrrhite, in Jameson's Edinburgh Journal, vol. xxix., the resemblance struck me. I therefore sacrificed these crystals to the blowpipe; they gave the following indications :—

The minutest transparent crystals changed immediately, in the reducing flame, to a deep, dull indigo blue, perfectly distinct; the edges then rounded, and, after considerable exposure, fused without intumescence; on the application of borax, the fusion was immediate, and a small, transparent, light brown bead remained. The largest crystal was then exposed to the outer flame; it became opake, of a light gray colour; before the reducing flame it changed apparently to black; but the blue colour is clearly seen, in a strong light, on the solid angles. Of this crystal the edges alone could be rounded by long exposure.

The hardness is about equal to felspar, and the form is that of the regular octahedron. Although this form is unknown among titanic minerals, the experiments of Kersten lead me to suppose that it belongs to this family.

Bucholzite and Xenolite.

In April 1843, I read a paper before this Society, which was published among their Proceedings, in which I asserted the identity of these two minerals. This has been confirmed by Rammelsberg, in his Supplement, published July 1843, from a consideration of the similarity of their chemical constituents. This question may therefore be considered as settled; but it is not probable that Rammelsberg had seen the paper alluded to above.

On the singular Crystals of Galena, figured in Alger's and in Dana's Mineralogy.

A good explanation of the formation of these singular crystals may be given as follows :—It is well known that a liquid globule of phosphate of lead, on cooling, from the action of the blowpipe, takes a polyhedral form, generally that of a rhombic or pentagonal dodecahedron.

A microscopic examination of this cooling process shows that, as the outer surface of the globule cools, the angles of the planes appear to start out from the circumference, the planes to flatten into their symmetrical shape; the uncooled liquid central portion, pressed by the contraction of the cooling exterior, oozes out from the middle of the plane, and spreads in a thin liquid plate over part of the surface, taking nearly the form of the plane; contraction still continuing, a succession of thin plates ooze out, each of course spreading somewhat short of its predecessor, but retaining the same form. This is probably the mode of the formation of these crystals of Galena, all of which bear the appearance of having undergone fusion. On many crystals of fluor spar from England, successive plates of this kind may be observed; many of them, however, do not take

their origin from the centre of the plane. If these, like those of phosphate of lead, are formed by the sudden cooling of heated solutions, this may easily be accounted for on the assumption that one side of the crystal was attached near the source of heat; the other side cooling more rapidly, the liquid inside oozed out nearer the more heated and still soft edge or plane. The crystals of phosphate of lead, on the contrary, cool equally on all sides. Forms of quartz with these plates are quite common; and I have recently found crystals of phosphate of lime, from Grafton, New Hampshire, with the same appearance. These plates have been considered as marks of cleavage lines; and it is evident that the cooling of each plate, previous to the superimposition of a fresh one, would cause less strength of adhesion between them than between other lines of the crystal.

Pyrochlore. (*Microlite.*)

The close examination of above 200 crystals of the mineral named microlite by Prof. Shepard, and the comparison of them with about 50 crystals of pyrochlore from the Swedish localities and from the Ural Mountains, resulting in their agreement in colour, cleavage, crystalline form and modifications, indicated to me, in 1841, the complete identity of the two minerals, although Wöhler's analysis had decreed the latter to be a titanate, while Shepard's had made the former a columbate of lime.

This identity, strenuously resisted by Prof. Shepard, although on grounds which show a very superficial knowledge of the whole subject, has been completely proved by subsequent analyses, particularly by that of A. A. Hayes, in Silliman's Journal, vol. xxxii. p. 341, and its station as a columbate of lime, according to one of Shepard's analyses, confirmed. Dana's Mineralogy, one of the arrangements of which is crystallographical, although in the last edition entering into every other possible detail on these two minerals, singularly enough omits even an allusion to the above circumstance, notwithstanding its being so remarkable an instance of the power of crystallography to indicate error in chemical analysis, even in hands like those of Wöhler.

This mineral is an excellent exemplification of the difficulties which at present surround the natural arrangement of minerals, although chemical analysis is unquestionably hereafter destined to be its basis. The analyses of the dark-coloured crystals give as ingredients, columbic acid, lime, manganese, iron, tin, lead, uranium, &c.; whereas the minute transparent yellow crystals are probably pure columbate of lime, or, perhaps, even obtaining their colour from a slight admixture of oxide of uranium, as this colour differs much in intensity in crystals of the same size. These small transparent crystals are generally modified on the edges and solid angles of the octahedron; in the large dark-coloured crystals these modifications are often nearly obliterated.

My largest crystal of pyrochlore, from the Chesterfield locality, is three-eighths of an inch at the base of the octahedral pyramid.

Columbite is usually described as of a dark, opake, sub-metallic, iron-black colour. I possess a small crystal of this substance, from

Chesterfield, of a brilliant, transparent, dark ruby red. I name this subject, because, as in the previous instance of pyrochlore, the small transparent crystals of minerals are usually most free from adulteration, and the fittest to produce the true atomic formula on chemical analysis. The excellent observations of Rammelsberg on this whole subject are well worth the attention of all mineralogists.

Pyrophyllite and Vermiculite.

In the paper before alluded to, published in the Proceedings of this Society, I have stated the probability of the identity of these two minerals.

Vermiculite is imbedded in a decomposed, magnesian, probably steatitic mass, of a light mealy appearance ; from this it is almost impossible completely to clear it. Thomson, no doubt, therefore analysed a considerable proportion of this substance with his specimen of vermiculite. Steatite contains but a mere trace of alumina ; hence his analysis would naturally give much less of this ingredient than Herrman's of pyrophyllite. Accordingly we find in Herrman $29\frac{1}{4}$ per cent. alumina, in Thomson only $7\frac{1}{4}$ per cent. On the other hand, steatite contains about 30 per cent. of magnesia. And here the same coincidence takes place. Herrman finds only 4 per cent. magnesia, and Thomson 17 per cent.; they also differ about 5 per cent. in the quantity of water. This is not surprising in a mineral whose singular character as to heat is supposed to arise from the mechanical existence of water between the laminæ, although I do not consider it a fact by any means proved that it is water alone. The ingredients of both analyses are the same, only differing in quantity, except the iron, which in one is peroxide, in the other protoxide. The difference in these two analyses I conceive far from being an objection to their identity. In vermiculite I have recently observed several laminæ of a light apple-green colour, agreeing with the original description of pyrophyllite by Herrman. Should future researches completely prove their identity, I believe vermiculite was first described. This name ought, therefore, properly to attach to the mineral ; but this is a subject of very trivial importance.

In the future progress of geology, when the lines and directions of paroxysmal action are better understood, a strict comparison of the inorganic ingredients of the elevated mountains and crystalline intrusions arising from periodical movements or convulsions of the central liquid mass, in places distant from each other, will certainly become a subject of considerable interest, and be invested with a character somewhat resembling that of the comparison of the organic ingredients of sedimentary deposits ; nor is it impossible that such investigations may eventually elicit data of great importance in geology.

For the generalizations of some future master spirit on this subject, the mineralogist of the present day is storing up facts and materials ; and it is from this prospect that a discovery of the identity of minerals from distant localities, and the purification of mineralogy from the numerous supposed new species with which the conceit or want of industry of its votaries have loaded it, becomes an object of some interest.

ON THE PREPARATION AND PROPERTIES OF CERTAIN CHLORATES. BY M. ALEXANDER WAECHTER.

Protochlorate of Iron.—A colourless neutral solution of this salt is obtained by decomposing a solution of protosulphate of iron with one of chlorate of barytes, both solutions being cold. This salt decomposes spontaneously when the solution is not kept at a very low temperature; it then loses its neutrality, precipitates basic perchloride of iron of a cinnamon colour, and there remains a deep red coloured solution of perchlorate and perchloride of iron.

Protochloride of Tin.—Hydrated protoxide of tin, when recently precipitated, dissolves in aqueous chloric acid, and forms a colourless solution; this decomposes spontaneously in a few minutes with strong detonation and increased temperature, yielding a gelatinous mass when the decomposition is over; this contains hydrated oxide of tin, perchloride of tin and much free chloric acid. The detonation arises from a decomposition of a portion of the chloric acid, produced by the increase of temperature.

Chlorate of Zinc.—The solution of this salt is obtained by the decomposition of chlorate of barytes by means of sulphate of zinc; by evaporation over sulphuric acid a very deliquescent crystalline mass was obtained, which was very soluble in alcohol; this melts at 140° Fahr., and at a little higher temperature it yields chlorine, oxygen and water; and when the heat is sufficiently raised, pure oxide of zinc remains.

It yielded by analysis,

Chloric acid ..	44·46
Oxide of zinc..	23·72
Water	31·82
	100·

Chlorate of Nickel.—A solution of this salt was obtained by decomposing chlorate of barytes with sulphate of nickel, and the solution yielded, by evaporation over sulphuric acid, very fine regular octahedrons, of a deep green colour, which very readily deliquesced in the air and were very soluble in alcohol; they melt in their water of crystallization at 176° Fahr., and begin to decompose at 284°, yielding water, chlorine and oxygen; if the temperature be not raised above 392°, a black mixture, a residue of peroxide and chloride of nickel, remains, which at a low red heat assumes a yellowish-gray colour, and is then a basic chloride of nickel. If it be heated to redness for a long time, it is converted into pure oxide of nickel of a silver-gray colour.

The crystals of chloride of nickel were found to be composed of

Chloric acid ..	45·16
Oxide of nickel	22·50
Water	32·34
	100·

Chlorate of Cobalt.—Chlorate of barytes was decomposed by sulphate of cobalt; the solution, separated by the filter from the sul-

phate of barytes, gave, by evaporation over sulphuric acid, regular octahedrons. The crystals are extremely deliquescent, and very soluble in alcohol; they melt at 122°, and are totally decomposed at 212°, with the evolution of chlorine, oxygen and water, leaving pure peroxide of cobalt.

This salt yielded by analysis,

Chloric acid	45·18
Oxide of cobalt..	22·48
Water	32·34
	100·

Chlorate of Copper.—The solution of this salt, obtained by the decomposition of chlorate of barytes and sulphate of copper, yielded by evaporation *in vacuo* over sulphuric acid, a dark green liquid of the consistence of a syrup, which became a crystalline mass by exposure to intense cold. Distinct crystals, which appeared to be regular octahedrons, were however rarely obtained. Chlorate of copper is very deliquescent and very soluble in alcohol; it fuses at 149°, and begins to decompose at a somewhat higher temperature; fused chlorate of copper does not solidify till the temperature is reduced to about 68°, even when it has not suffered the slightest decomposition by heat; if it be heated to 212°, bubbles of gas are disengaged, each of which occasions a slight detonation. When the decomposition is effected at the lowest possible temperature, a green residue is obtained, which suffers no further decomposition till heated to above 500°; this is insoluble in water, but very soluble in dilute acids; the solution in weak nitric acid gives no precipitate with nitrate of silver; the solution in hydrochloric acid has a very distinct smell of chlorine; this green substance is therefore a basic chlorate of hydrate of copper.

Chlorate of copper yielded by analysis,

Chloric acid	44·60
Oxide of copper..	23·45
Water	31·95
	100·

Chlorate of Lead.—A neutral solution of this salt is obtained by saturating chloric acid with the oxide; its concentration may be continued over the spirit-lamp, without decomposition, until it begins to crystallize on cooling; the crystals are rhombic prisms; they are at first transparent and possess much lustre, but by exposure to the air they quickly become dull and opake; they do not deliquesce, but they dissolve very readily in water and in alcohol. When mixed with combustible bodies, they detonate by percussion almost as strongly as the potash salt. At 302° the crystals lose 4·59 per cent. of water; at 446° they are suddenly decomposed with a hissing noise, yielding chlorine and oxygen, and a partly fused black mass remains, which is a mixture of peroxide and chloride of lead; if the heat be continued this yields oxygen, and is converted into basic-yellow chloride of lead, the composition of which is constant.

This salt is composed of

Chloric acid ..	38·49
Oxide of lead..	56·92
Water	4·59
	100·

Chlorate of Silver.—A neutral solution of this salt is obtained by dissolving oxide of silver in chloric acid; this yields by evaporation distinct prismatic crystals; chlorate of silver is anhydrous; it dissolves in about five parts of cold water, and likewise in alcohol without suffering alteration; it fuses at 446°, and begins at 518° to give off oxygen with a trace of chlorine; when heated until the evolution of gas ceases, chloride of silver remains. The evolution of oxygen is much more ready and rapid if the salt be mixed with oxide of copper or peroxide of manganese. If chlorate of silver be heated very rapidly, it decomposes suddenly with explosion and the extrication of light. When mixed with combustible bodies, it detonates by percussion much more strongly than the potash salt.

It is composed of

Chloric acid ..	39·37
Oxide of silver	60·63
	100·

If chlorine be passed through water containing oxide of silver in suspension, chlorate of silver is also formed; but if the current of chlorine be too long continued, this salt decomposes and oxygen gas is evolved, and there are formed chloride of silver and free chloric, not perchloric acid. By the action of hypochlorous acid on oxide of silver, or also upon finely divided metallic silver, no chloric acid is formed, but a mixture of peroxide of silver and chloride of silver.

Ammoniacal Chlorate of Silver.—Prismatic crystals of this salt are obtained by dissolving chlorate of silver in ammonia, and evaporating the solution; these crystals are very soluble in water and in alcohol; they melt at 212° and lose their ammonia, and when the temperature does not exceed 534° pure chlorate of silver remains; when heated rapidly they decompose with explosion and the emission of light. The solution of this salt yields, on the addition of potash, a gray precipitate of Berthollet's fulminating silver.

This salt is composed of

|	
Chlorate of silver	84·81
Ammonia	15·19
	100·

Journ. de Pharm. et de Ch., Mai 1844.

ON THE PROPERTIES AND COMPOSITION OF THE PROTOXIDE OF GOLD. BY M. L. FIGUIER.

The modes of preparing the protoxide of gold proposed by the chemist above named were stated in the last Number of the Philosophical Magazine; we proceed now to state the

Properties of the Protoxide of Gold.—In the state of hydrate it is of so dark a violet colour as to appear black; when dried it has the violet-blue colour of the powder of Cassius. It does not decompose

below 482° Fahr., and it then yields oxygen; it is insoluble in water, and is not either dissolved or decomposed by alcohol. Hydrochloric acid converts it into metallic gold which separates, and terchloride of gold which dissolves; this reaction requires a quarter of an hour to complete it when the acid is cold, but at a boiling heat it occurs instantaneously; in the former case protochloride of gold is at first formed, which itself afterwards decomposes by the usual reaction on the contact of water,

$$3(Au^2Cl) = Au^2Cl^2 + Au^4.$$
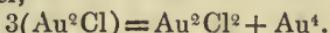
Hydriodic acid when brought into contact with protoxide of gold becomes of a deep yellow colour, and deposits protiodide of gold of a greenish colour. The supernatant liquor is a solution of protiodide of gold in hydriodic acid. If the whole be boiled, the iodine is dis-engaged in the form of violet vapours, and the gold is precipitated. There is not therefore produced, under these circumstances, a per-iodide of gold corresponding to the perchloride.

Hydrobromic acid acts like hydriodic acid; it gives when cold a brown deposit of bromide and becomes of a deep colour; if it be boiled the liquor becomes quite light-coloured; *aqua regia* dissolves the protoxide immediately, but neither sulphuric, nitric, nor acetic acid has any action upon it; potash and soda dissolve it only when in the nascent state; ammonia forms a violet fulminating compound; if rubbed with a hard body it explodes; but detonation is not so loud as that of the fulminating gold obtained with the teroxide of gold; hydrogen gas easily reduces it when hot, but without any sensible phænomenon.

Protoxide of gold ought to be arranged with indifferent chemical compounds, since it is capable of combining both with acids and bases; thus, while potash dissolves it in the nascent state (for ex-ample, during the treatment of the protochloride with this alkali) we shall find, on the other hand, that stannic acid readily combines with it.

The extraordinary changeableness which Berzelius supposed to be a property of this protoxide, does not belong to it. It is in fact much less alterable than the teroxide of gold, which is a usual cir-cumstance, for the first degree of oxidizement of a metal is in general more stable than the subsequent. Thus the protoxide of gold does not decompose below 482° Fahr., whilst the teroxide undergoes it at 437°.

Direct light, which reduces the teroxide of gold after a certain time, has no action on the protoxide; there are, moreover, a great number of chemical agents which easily reduce the teroxide of gold, that have no action whatever on the protoxide; such for example are tartaric acid, acetic acid and alcohol.

When protoxide of gold has been dried, it is perfectly insoluble in water; but if at the moment of its preparation it be put into contact with distilled water, there is then produced what is called apparent solution; thus when an attempt is made to wash on a filter the prot-oxide obtained from the protochloride of gold by potash, it may be observed that a small portion of the product is carried off by the washing water, which passes through of a deep violet-blue colour.

This liquor, when not strong, has the colour of weak tincture of litmus, but when concentrated it appears black; it remains clear for two or three days, but after this the oxide is entirely precipitated, and remains at the bottom of the vessel in the form of a blue cloud. The addition of a soluble salt to this liquor, when heated, occasions the instantaneous separation of the oxide, and a few drops of hydrochloric acid soon render it bright.

It does not always happen that distilled water produces the above described appearance, but the cause of the variation is not apparent; the mode of solution appears, however, to be similar to that of iodide of starch, prussian blue, &c. &c.—*Ann. de Ch. et de Phys.*, Juillet 1844.

PARABOLIC ELEMENTS OF THE COMET DISCOVERED BY M. VICTOR MAUVAIS, JULY 7, 1844, AS COMPUTED BY HIMSELF.

Time of perihelion passage, October 1844   14·7681 Paris mean time.
Logarithm of perihelion distance . . . . . .   9·8817875 ($q=0·7617$)
Longitude of perihelion. . . . . $176°\,35'\,42''$
Longitude of the ascending node 35 37 42
Inclination 49 41 23
Motion . . . .   retrograde

Comptes Rendus, 15 Juillet, 1844.

METEOROLOGICAL OBSERVATIONS FOR JULY 1844.

Chiswick.—July 1. Slight haze: cloudy: thunder, with rain from 6 till 8 P.M. 2. Rain: cloudy. 3. Cloudy. 4. Slight rain: cloudy. 5, 6. Fine. 7. Hazy and mild: overcast. 8. Hazy: very fine. 9—11. Very fine. 12. Very fine: showery. 13. Rain: heavy rain at night. 14, 15. Clear and fine. 16. Overcast. 17. Fine: dusky clouds: hazy. 18. Very fine: cloudy. 19. Overcast: thunder quarter to 1 P.M.: thunder-showers in afternoon. 20, 21. Clear and fine. 22. Cloudless and very hot. 23, 24. Sultry. 25. Very hot. 26. Cloudy: clear. 27. Hot and dry. 28. Very fine: cloudy. 29. Hot and very dry: exceedingly clear at night. 30. Overcast: rain. 31. Cloudy and fine: clear.—Mean temperature of the month 1° above the average.

Boston.—July 1. Fine: rain A.M. 2. Cloudy. 3. Fine. 4. Fine: rain P.M. 5. Rain. 6—8. Cloudy. 9—11. Fine. 12. Fine: rain A.M. 13. Fine: rain A.M. and P.M. 14. Stormy. 15. Fine: rain A.M., with thunder. 16. Fine: rain P.M. 17. Fine. 18. Fine: rain P.M., with thunder and lightning. 19. Fine: rain P.M. 20, 21. Fine. 22. Fine: thermometer 81° 2 o'clock P.M. 23. Fine: thermometer 80° 3 o'clock P.M., in the sun 112°. 24. Cloudy: rain A.M. 25. Fine: rain P.M. 26, 27. Cloudy. 28. Fine. 29. Fine: rain early A.M.: rain P.M. 30. Cloudy: rain P.M. 31. Cloudy: rain, with thunder and lightning A.M.

Sandwick Manse, Orkney.—July 1. Cloudy. 2—6. Drizzle: showers. 7—9. Cloudy. 10. Cloudy: showers. 11. Showers: cloudy. 12. Damp: cloudy. 13. Bright: cloudy. 14. Cloudy. 15. Cloudy: clear. 16. Bright: clear. 17. Clear. 18. Clear: showers. 19. Cloudy: showers: drizzle. 20. Cloudy. 21. Cloudy: showers. 22. Cloudy: fine. 23. Clear: fine. 24. Bright: cloudy. 25. Clear: fine. 26. Clear: fog. 27. Clear: fine. 28. Bright: showers: fog. 29. Drizzle. 30. Cloudy. 31. Rain.

Applegarth Manse, Dumfries-shire.—July 1. Fine: a few drops of rain. 2—4. Fine: sultry. 5. Fine, but cloudy. 6. Slight shower. 7. Fine and fair. 8. Fine: a few drops of rain. 9. Fine, but cloudy. 10. Showers. 11. One shower. 12. A few drops of rain. 13. Heavy rain. 14. Showers: thunder. 15—17. Showers. 18. Showers: thunder. 19. Slight shower: thunder. 20. Fair. 21. Wet all day. 22. Fine. 23. One heavy shower: thunder. 24—26. Rain: heavy P.M. 27. Beautiful day. 28. Showers. 29. Fair. 30, 31. Rain.

Meteorological Observations made by Mr. Thompson at the Garden of the Horticultural Society at Chiswick, near London; by Mr. Veall, at Boston; by the Rev. W. Dunbar, at Applegarth Manse, Dumfries-shire; and by the Rev. C. Clouston, at Sandwick Manse, Orkney.

Days of Month 1844 July	Bar. Chiswick Max.	Bar. Chiswick Min.	Bar. Boston 8½ a.m.	Bar. Dumfries 9 a.m.	Bar. Dumfries 9 p.m.	Bar. Orkney Sandwick 9½ a.m.	Bar. Orkney Sandwick 8½ p.m.	Therm. Chiswick Max.	Therm. Chiswick Min.	Therm. Boston 8½ a.m.	Therm. Dumfries Max.	Therm. Dumfries Min.	Therm. Orkney 9½ a.m.	Therm. Orkney 8½ p.m.	Wind Chiswick 1 p.m.	Wind Boston	Wind Dumfries-shire	Wind Orkney Sandwick	Rain Chiswick	Rain Boston	Rain Dumfries-shire	Rain Orkney Sandwick
1.	29·853	29·829	29·39	29·77	29·77	29·90	29·90	79	54	57	67	47	49½	49	s.	calm	se.	n.	·34			
2.	29·841	29·832	29·37	29·80	29·80	29·91	29·91	69	53	60	63½	49	52	47	n.	calm	sse.	n.	·22	·07		·13
3.	29·876	29·809	29·43	29·79	29·73	29·90	29·90	72	57	62	67	50	48½	47½	se.	calm	s.	nne.	·14			
4.	29·638	29·596	29·19	29·69	29·67	29·84	29·78	68	55	63	60	49	52	48	sw.	calm	ne.	nw.	·01			
5.	29·656	29·576	29·13	29·65	29·63	29·73	29·70	70	50	58	64	49½	49	47	ne.	calm	se.	n.	·06	·79		
6.	29·878	29·809	29·34	29·71	29·80	29·80	29·92	65	54	60	64	53	48	46	n.	calm	s.	nw.	·06			·06
☽7.	29·995	29·929	29·50	29·88	29·85	29·95	29·95	68	53	57	67	52	48½	46½	e.	sw.	s.	nw.	·13		0·06	
8.	29·931	29·856	29·47	29·84	29·78	29·88	29·84	77	58	55	66	46	55	49	w.	w.	w.	se.				
9.	29·906	29·850	29·34	29·76	29·81	29·87	29·87	74	49	64	62½	49	53	51½	n.	w.	w.	w.				
10.	29·967	29·880	29·44	29·81	29·66	29·76	29·62	78	60	65	63	47	59½	52	sw.	calm	w.	ssw.				·25
11.	29·880	29·864	29·29	29·67	29·65	29·44	29·54	78	56	64·5	63½	51	52	50½	w.	w.	sw.	w.	·06	·08		·05
12.	29·939	29·870	29·27	29·72	29·78	29·64	29·74	78	56	63	58½	50	51½	48½	w.	w.	wnw.	wnw.	·53	·36		
13.	29·905	29·494	29·33	29·65	29·17	29·64	29·74	71	56	62	62	48	56	51	sw.	calm	sse.	se.				
14.	29·679	29·530	28·88	29·20	29·59	29·44	29·64	75	47	62	61	51	53	49	w.	w.	s.	n.			1·10	
●15.	29·928	29·809	29·30	29·60	29·70	29·71	29·82	76	46	65·5	63	41½	52	50	w.	w.	w.sw.	nw.				
16.	29·994	29·949	29·47	29·82	29·89	29·87	29·93	75	42	62	64	42	51	49	sw.	calm	sw.	.....		·16		·09
17.	30·023	29·848	29·50	29·87	29·76	29·92	29·93	73	52	62	59	48½	57	46	n.	calm	w.	se.		·34		
18.	29·752	29·718	29·23	29·65	29·60	29·73	29·72	73	45	63	63	46½	54	47½	w.	calm	sw.nw	n.				
19.	29·891	29·723	29·24	29·68	29·80	29·76	29·92	74	44	59	65	42	52½	50	nw.	calm	sw&s.	n.	·39	·29		
20.	30·174	30·067	29·58	29·98	30·09	30·08	30·17	76	43	60	59½	48½	52½	54	s.	calm	se.sw	e.		·15		·03
21.	30·275	30·067	29·76	29·99	30·07	30·08	30·01	82	49	63	68½	56	52½	60	sw.	calm	s.	ssw.				
☽22.	30·190	30·044	29·59	30·06	29·97	29·96	30·00	89	53	72	76	50	67	58½	e.	calm	ese.	ese.			0·76	·06
23.	29·999	29·910	29·40	30·03	29·83	29·98	30·00	89	61	75	76	49½	68	55	e.	s.	w.	sse.				
24.	29·948	29·916	29·33	29·90	29·90	29·98	29·98	87	53	67	75	59½	62½	56½	s.	calm	se.	ese.				
25.	29·956	29·901	29·35	29·87	29·84	30·00	30·00	92	62	70·5	66	58	61½	55	nw.	calm	ne.	nw.		·05		
26.	30·110	29·908	29·30	29·91	30·03	30·02	30·13	74	55	67	71½	51	60	55½	nw.	w.	sse.	nw.		·05		
27.	30·181	30·161	29·55	30·10	30·11	30·15	30·14	83	50	65	68½	57½	57	52½	nw.	calm	ssw.	sse.				
28.	30·197	30·005	29·59	30·05	29·89	30·04	29·92	88	57	70	67	50½	64	52½	sw.	n.	ssw.	nw.				
○29.	29·965	29·947	29·38	29·75	29·75	29·88	29·57	76	44	62	59½	48	53	52½	nw.	calm	ssw.	se.		·10		·37
30.	29·828	29·474	29·24	29·56	29·20	29·64	29·56	68	55	60	59	53	56	53	sw.	nw.	sse.	nw.	·14	·03	2·05	
31.	29·708	29·529	28·94	29·45	29·47	29·58		72	49	60			52½		w.				·02	·16		·13
Mean.	29·937	29·833	29·35	29·780	29·760	29·841	29·850	76·38	52·19	63·0	64·8	47·9	54·83	51·04					2·10	2·63	3·97	1·17

XXXIX. *On Quaternions; or on a new System of Imaginaries in Algebra. By* Sir WILLIAM ROWAN HAMILTON, *LL.D., P.R.I.A., F.R.A.S., Hon. M. R. Soc. Ed. and Dub., Hon. or Corr. M. of the Royal or Imperial Academies of St. Petersburgh, Berlin, Turin, and Paris, Member of the American Academy of Arts and Sciences, and of other Scientific Societies at Home and Abroad, Andrews' Prof. of Astronomy in the University of Dublin, and Royal Astronomer of Ireland.*

6. IT follows immediately from the principles already explained, that if R R' R'' be any spherical triangle, and if $\alpha \beta \gamma$ be the rectangular co-ordinates of R, $\alpha' \beta' \gamma'$ of R', and $\alpha'' \beta'' \gamma''$ of R'', the centre O of the sphere being origin and the radius unity, and the positive semiaxis of z being so chosen as to lie to the right or left of the positive semiaxis of y, with respect to the positive semiaxis of x, according as the radius OR'' lies to the right or left of OR' with respect to OR, then the following imaginary or symbolic *formula of multiplication* of quaternions will hold good:

$$\{\cos R + (i\alpha + j\beta + k\gamma)\sin R\}\{\cos R' + (i\alpha' + j\beta' + k\gamma')\sin R'\}$$
$$= -\cos R'' + (i\alpha'' + j\beta'' + k\gamma'')\sin R''; \quad \cdot \quad \cdot \quad \cdot \quad \text{(I.)}$$

the squares and products of the three imaginary units, i, j, k, being determined by the nine equations of definition, assigned in a former article, namely,

$$i^2 = j^2 = k^2 = -1; \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \text{(A.)}$$
$$ij = k, \; jk = i, \; ki = j; \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \text{(B.)}$$
$$ji = -k, \; kj = -i, \; ik = -j. \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \text{(C.)}$$

Developing and decomposing the imaginary formula (I.) by these conditions, it resolves itself into the four following real equations of spherical trigonometry:

$$-\cos R'' = \cos R \cos R' - (\alpha\alpha' + \beta\beta' + \gamma\gamma')\sin R \sin R',$$
$$\alpha''\sin R'' = \alpha\sin R \cos R' + \alpha'\sin R'\cos R + (\beta\gamma' - \gamma\beta')\sin R \sin R', \left.\right\} \text{(K.)}$$
$$\beta''\sin R'' = \beta\sin R \cos R' + \beta'\sin R'\cos R + (\gamma\alpha' - \alpha\gamma')\sin R \sin R',$$
$$\gamma''\sin R'' = \gamma\sin R \cos R' + \gamma'\sin R'\cos R + (\alpha\beta' - \beta\alpha')\sin R \sin R';$$

of which indeed the first answers to the well-known relation
(already employed in this paper), connecting a side with the
three angles of a spherical triangle. The three other equa-
tions (K.) correspond to and contain a theorem (perhaps new),
which may be enunciated thus: that if three forces be applied
at the centre of the sphere, one equal to sin R cos R' and di-
rected to the point R, another equal to sin R' cos R and di-
rected to R', and the third equal to sin R sin R' sin R R' and
directed to that pole of the arc R R' which lies towards the
same side of this arc as the point R'', the resultant of these
three forces will be directed to R'', and will be equal to sin R''.
It is not difficult to prove this theorem otherwise, but it may
be regarded as interesting to see that the four real equations
(K.) are all included so simply in the single imaginary formula
(I.), and can so easily be deduced from that formula by the
rules of the *multiplication of quaternions*; in the same manner
as the fundamental theorems of plane trigonometry, for the
cosine and sine of the sum of any two arcs, are included in
the well-known formula for the multiplication of *couples*, that
is, expressions of the form $x + iy$, or more particularly, $\cos\theta$
$+ i\sin\theta$, in which $i^2 = -1$. A new sort of algorithm, or *cal-
culus for spherical trigonometry*, would seem to be thus given
or indicated.

  And if we suppose the spherical triangle R R' R'' to become
indefinitely small, by each of its corners tending to coincide
with the point of which the co-ordinates are 1, 0, 0, then each
co-ordinate α will tend to become $=1$, while each β and γ will
ultimately vanish, and the sum of the three angles will ap-
proach indefinitely to the value π; the formula (I.) will there-
fore have for its limit the following,

$$(\cos R + i\sin R)(\cos R' + i\sin R') = \cos(R + R') + i\sin(R + R'),$$

which has so many important applications in the usual theory
of imaginaries.

  7. In that theory there are only two different square roots
of negative unity, and they differ only in their signs. In the
theory of quaternions, in order that the square of $w + ix + jy$
$+ kz$ should be equal to -1, it is necessary and sufficient
that we should have

$$w = 0, \quad x^2 + y^2 + z^2 = 1;$$

for, in general, by the expressions (D.) of this paper for the

constituents of a product, or by the definitions (A.), (B.), (C.), we have

$$(w + ix + jy + kz)^2 = w^2 - x^2 - y^2 - z^2 + 2w(ix + jy + kz).$$

There are, therefore, in this theory, *infinitely many different square roots of negative one,* which have all one common modulus $= 1$, and one common amplitude $= \dfrac{\pi}{2}$, being all of the form

$$\sqrt{-1} = i \cos \phi + j \sin \phi \cos \psi + k \sin \phi \sin \psi, \quad . \quad (L.)$$

but which admit of all varieties of *directional co-ordinates,* that is to say, co-latitude and longitude, since ϕ and ψ are arbitrary; and we may call them all *imaginary units,* as well as the three original imaginaries, i, j, k, from which they are derived. To distinguish one such root or unit from another, we may denote the second member of the formula (L.) by $i_{\phi, \psi}$, or more concisely by i_{R}, if R denote (as before) that particular point of the spheric surface (described about the origin as centre with a radius equal to unity) which has its co-latitude $= \phi$, and its longitude $= \psi$. We shall then have

$$i_{\mathrm{R}} = i\,\alpha + j\,\beta + k\,\gamma, \; i_{\mathrm{R}}^2 = -1, \quad . \quad . \quad . \quad (\mathrm{L}'.)$$

in which

$$\alpha = \cos \phi, \; \beta = \sin \phi \cos \psi, \; \gamma = \sin \phi \sin \psi,$$

α, β, γ being still the rectangular co-ordinates of R, referred to the centre as their origin. The formula (I.) will thus become, for any spherical triangle,

$$(\cos \mathrm{R} + i_{\mathrm{R}} \sin \mathrm{R})(\cos \mathrm{R}' + i_{\mathrm{R}'}\sin \mathrm{R}') = -\cos \mathrm{R}'' + i_{\mathrm{R}''}\sin \mathrm{R}''. \; (\mathrm{I}'.)$$

8. To *separate the real and imaginary parts* of this last formula, it is only necessary to effect a similar separation for the product of the two imaginary units which enter into the first member. By changing the angles R and R′ to right angles, without changing the points R and R′ upon the sphere, the imaginary units i_{R} and $i_{\mathrm{R}'}$ undergo no change, but the angle R″ becomes equal to the arc R R′, and the point R″ comes to coincide with the *positive pole* of that arc, that is, with the pole to which the least rotation from R′ round R is positive. Denoting then this pole by P″, we have the equation

$$i_{\mathrm{R}} i_{\mathrm{R}'} = -\cos \mathrm{R}\,\mathrm{R}' + i_{\mathrm{P}''} \sin \mathrm{R}\,\mathrm{R}', \; . \quad . \quad . \quad (\mathrm{M}.)$$

which is included in the formula (I′.), and reciprocally serves to transform it; for it shows that while the comparison of the real parts reproduces the known equation

$$\cos \mathrm{R} \cos \mathrm{R}' - \sin \mathrm{R} \sin \mathrm{R}' \cos \mathrm{R}\,\mathrm{R}' = -\cos \mathrm{R}'', \; . \quad (\mathrm{K}'.)$$

the comparison of the imaginary parts conducts to the following symbolic expression for the theorem of the 6th article:

$$i_R \sin R \cos R' + i_{R'} \sin R' \cos R + i_{P''} \sin R \sin R' \sin R R' = i_{R''} \sin R''. \quad (K''.)$$

As a verification we may remark, that when the triangle (and with it the arc R R') tends to vanish, the two last equations tend to concur in giving the property of the plane triangle,

$$R + R' + R'' = \pi.$$

9. The expression (M.) for the *product of any two imaginary units*, which admits of many applications, may be immediately deduced from the fundamental definitions (A.), (B.), (C.), respecting the squares and products of the three original imaginary units, i, j, k, by putting it under the form

$$(i\alpha + j\beta + k\gamma)(i\alpha' + j\beta' + k\gamma') =$$
$$-(\alpha\alpha' + \beta\beta' + \gamma\gamma') + i(\beta\gamma' - \gamma\beta') + j(\gamma\alpha' - \alpha\gamma') + k(\alpha\beta' - \beta\alpha'); (M'.)$$

and it is evident, either from this last form or from considerations of rotation such as those already explained, that if the *order* of any two pure imaginary factors be changed, the real part of the product remains unaltered, but the imaginary part changes sign, in such a manner that the equation (M.) may be combined with this other analogous equation,

$$i_{R'} i_R = -\cos R R' - i_{P''} \sin R R'. \quad . \quad . \quad (N.)$$

In fact, we may consider $-i_{P''}$ as $= i_{P_l''}$, if P_l'' be the point diametrically opposite to P'', and consequently the positive pole of the reversed arc R' R (in the sense already determined), though it is the *negative pole* of the arc R R' taken in its former direction.

And since in general the product of any two quaternions, which differ only in the signs of their imaginary parts, is real and equal to the square of the modulus, or in symbols,

$$\mu(\cos\theta + i_R \sin\theta) \times \mu(\cos\theta - i_R \sin\theta) = \mu^2, \quad . \quad . \quad (O.)$$

we see that the product of the two different products, (M.) and (N.), obtained by multiplying any two imaginary units together in different orders, is real and equal to unity, in such a manner that we may write

$$i_R i_{R'} . i_{R'} i_R = 1; \quad . \quad . \quad . \quad . \quad . \quad (P.)$$

and the two quaternions, represented by the two products $i_R i_{R'}$ and $i_{R'} i_R$, may be said to be *reciprocals* of each other. For example, it follows immediately from the fundamental definitions (A.), (B.), (C.), that

$$ij . ji = k \times -k = -k^2 = 1;$$

the products ij and ji are therefore reciprocals, in the sense just now explained. By supposing the two imaginary factors,

i_R and $i_{R'}$, to be mutually *rectangular*, that is, the arc R R′ = a quadrant, the two products (M.) and (N.) become $\pm i_{P''}$; and thus, or by a process more direct, we might show that if two imaginary units be mutually *opposite* (one being the negative of the other), they are also mutually *reciprocal*.

10. The equation (P.), which we shall find to be of use in the *division of quaternions,* may be proved in a more purely algebraical way, or at least in one more abstracted from considerations of directions in space, as follows. It will be found that, in virtue of the definitions (A.), (B.), (C.), every equation of the form

$$\iota \cdot \varkappa \, \lambda = \iota \, \varkappa \cdot \lambda$$

is true, if the three factors, $\iota, \varkappa, \lambda$, whether equal or unequal among themselves, be equal, each, to one or other of the three imaginary units i, j, k; thus, for example,

$$i \cdot jk = (i \cdot i = -1 = k \cdot k =) ij \cdot k,$$
$$j \cdot ji = (j \cdot -k = -i = -1 \cdot i =) jj \cdot i.$$

Hence, whatever three quaternions may be denoted by Q, Q′, Q″, we have the equation

$$Q \cdot Q' Q'' = Q Q' \cdot Q''; \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad (Q.)$$

and in like manner, for any four quaternions,

$$Q \cdot Q' Q'' Q''' = Q Q' \cdot Q'' Q''' = Q Q' Q'' \cdot Q''', \quad \cdot \quad (Q'.)$$

and so on for any number of factors; the notation $Q Q' Q''$ being employed, in the formula (Q′.), to denote that one determined quaternion which, in virtue of the theorem (Q.), is obtained, whether we first multiply Q″ as a multiplicand by Q′ as a multiplier, and then multiply the product Q′ Q″ as a multiplicand by Q as a multiplier; or multiply first Q′ by Q, and then Q″ by Q Q′. With the help of this principle we may easily prove the equation (P.), by observing that

$$i_R \, i_{R'} \cdot i_{R'} \, i_R = i_R \cdot i_{R'}^2 \, i_R = -i_R^2 = +1.$$

11. The theorem expressed by the formulæ (Q.), (Q′.), &c., is of great importance in the *calculus of quaternions,* as tending (so far as it goes) to assimilate this system of calculations to that employed in ordinary algebra. In ordinary multiplication we may distribute any factor into any number of parts, real or imaginary, and collect the partial products; and the same process is allowed in operating on quaternions: *quaternion-multiplication* possesses therefore the *distributive* character of multiplication commonly so called, or in symbols,

$$Q(Q' + Q'') = Q Q' + Q Q'', \quad (Q + Q') Q'' = Q Q'' + Q' Q'', \&c.$$

But in ordinary algebra we have also

$$Q Q' = Q' Q;$$

which equality of products of factors, taken in opposite orders, does *not* in general hold good for quaternions $(j\,i = -i\,j)$; the *commutative* character of ordinary multiplication is therefore in general *lost* in passing to the new operation, and $Q\,Q' - Q'Q$, instead of being a symbol of zero, comes to represent a pure imaginary, but not (in general) an evanescent quantity.   On the other hand, for quaternions as for ordinary factors, we *may* in general *associate the factors among themselves, by groups, in any manner which does not alter their order*; for example,

$$Q \cdot Q'\,Q'' \cdot Q'''\,Q^{IV} = QQ' \cdot Q''\,Q'''\,Q^{IV};$$

this, therefore, which may be called the *associative* character of the operation, is (like the distributive character) *common* to the multiplication of quaternions, and to that of ordinary quantities, real or imaginary.

<div align="center">[To be continued.]</div>

XL. *Additional Researches on the Electrolysis of Secondary Compounds.*   By J. FREDERIC DANIELL, *Esq.*, *D.C.L.*, *For. Sec. R.S.*, *Prof. of Chem. in King's College, London,* and W. A. MILLER, *Esq.*, *M.D.*, *Dem. of Chem. in King's College, London.*

<div align="center">[Continued from p. 188 and concluded.]</div>

6. WE were purposing to follow up the examination of the double salts by the electrolysis of other classes of these compounds, when we were stopped by the occurrence of a new order of phænomena, which it was necessary to turn aside and examine as affecting the very foundation of the process which we were employing.   In our examination of the phosphates, our plan had been to transfer by electrolysis the acid of the salt placed in the platinode cell into a solution of potassa placed in the zincode cell for its reception, and then to determine its quantity and its properties from those of the salt which was formed by secondary combination.

The arrangement which we now made was to place the salt at the zincode with the view of passing its bases into an acid at the platinode.   We commenced our experiments with the double sulphates with the view of comparing the different hydrates of the sulphuric acid with the hydrates of the phosphoric acid already examined, and throwing more light upon the results of the electrolysis of the dilute acid.

(*s.*) Some pure *sulphate of potassa and copper* (CuO, SO_3, $+ KO, SO_3$) was carefully prepared, and its solution placed in the zincode and connecting cells of the diaphragm apparatus: the platinode cell was charged with dilute sulphuric acid, the current of twenty cells of the battery was transmitted

through the arrangement until 48 cubic inches of hydrogen had been collected from the platinode, and 23·5 cubic inches of oxygen from the zincode (about one equivalent in grains), when the process was stopped. The platinode solution was perfectly colourless with a very slight deposit of copper upon the platinode, amounting only to 0·1 grain. It was evaporated to dryness in vapour of ammonia, and the residue was perfectly white sulphate of potassa, which weighed 21 grains. It was entirely soluble in water: and ammonia did not produce the slightest blue colour in its solution. The liquids of the zincode and connecting tubes together required 51 grains of dry carbonate of soda to neutralize the free acid. Now, it will be observed, that the sulphate of potassa was only one grain less than a quarter of an equivalent, which, considering the nature of the processes by which these quantities were determined, will appear a very near approximation; and we may take the results of the electrolysis to have been as follows:— For one equivalent of force as measured by the gases evolved at the zincode and platinode, half an equivalent of oxysulphion of potassium (sulphate of potassa) was decomposed (as measured by the quarter equivalent potassium transferred), and half an equivalent of oxysulphion of hydrogen (aqueous sulphuric acid): for it will be observed, as most remarkable, that the quantity of copper transferred was perfectly insignificant.

Upon the view of the principle of electrolysis hitherto commonly admitted, viz. definite decomposition with equivalent and opposite transfer of the elements or radicles of the compound to the opposite electrodes of the battery, these results were sufficiently perplexing: but our perplexity increased as we proceeded; and it was not till after we had expended an incredible amount of labour in testing different hypotheses of various resistances and opposing forces, supposed to be developed in these complicated cases, that we thought of questioning the principles themselves. These abortive attempts we shall not, of course, obtrude upon the Society, but proceed to select such experimental evidence as may be sufficient to establish, in as concise a manner as possible, the conclusions to which we have arrived.

(*t.*) A solution of *sulphate of alumina and potassa* (alum) $(Al_2 O_3, 3SO_3) + (KO, SO_3)$ was subjected to exactly the same treatment as the preceding double salt, and the results were precisely similar. 48 cubic inches of hydrogen were collected from the platinode. When ammonia was added in excess to the solution in the platinode cell no precipitate was produced, which proved the absence of alumina. Evaporated to dry-

ness, the saline residue was ignited, and weighed 21 grains. It was sulphate of potassa, and the amount, as before, was within one grain of the quarter of an equivalent.

(*u.*) Some of the double *sulphate of magnesia and potassa* ($MgO, SO_3, + KO, SO_3$) was carefully prepared, and submitted in solution to the same treatment. After 48 cubic inches of hydrogen had been collected the platinode solution was evaporated to dryness, and left a saline residue of 36·6 grains. This was redissolved and treated with carbonate of potassa. A precipitate of carbonate of magnesia was thus obtained, which was reconverted into sulphate, and weighed 3·8 grains; this was equivalent to 0·76 grain of magnesium, to which must be added 0·24 grain recovered from the wash water by means of phosphate of ammonia, making the total quantity of magnesium which had passed over to the platinode 1·0, or about $\frac{1}{12}$th of an equivalent. Deducting the total amount of sulphate of magnesia, 4·98 grains, from the mixed sulphates, 36·6 grains, we have 31·62 as the amount of the sulphate of potassa.

Thus it appeared that the three bases, alumina, magnesia, and oxide of copper, or their metals, when in combination with sulphuric acid and potassa, refused to travel from the zincode to the platinode under the influence of the voltaic current into an acid with which they were capable of forming an immediate and soluble combination. Could it be that the same results might follow from the electrolysis of the single salts of the same acid?

(*w.*) Some pure *sesquisulphate of alumina* ($Al_2 O_3, 3SO_3$) was prepared by precipitating alum with excess of ammonia, thoroughly washing the precipitate and redissolving it in sulphuric acid, with the aid of heat. An excess of alumina was left, from which the liquid was separated by filtration. It was then evaporated till it solidified upon cooling. The mass was subjected to pressure in calico, and the dry salt dissolved in water.

The zincode and connecting cells of the diaphragm apparatus were then charged with the solution, and the platinode cell with dilute sulphuric acid. The arrangement conducted well, and in half an hour 13 cubic inches of oxygen were collected from the zincode. The process was then stopped, and on supersaturating the liquid at the platinode with ammonia, the slightest possible trace of alumina was precipitated.

Thus it again appeared that the electrolysis was carried on without the transfer of the base or its metal to the platinode.

Was the peculiar constitution of this salt as a *sesqui* compound concerned in any way in this result?

(*x.*) A salt of the same order, viz. the *sesquisulphate of iron*

($Fe_2 O_3$, $3SO_3$), was prepared with great care, and its solution treated exactly in the same way as the last. It conducted well, and 13 cubic inches of oxygen were collected from the zincode in twenty-eight minutes. On neutralizing the liquid of the platinode cell with ammonia, very slight traces of sesquioxide of iron appeared. The sesquioxide of iron had therefore resisted the transfer as well as the sesquioxide of aluminum.

(*y*.) We next tried salts of the more ordinary constitution of single equivalents of acid and base. The zincode and connecting cells were charged with solution of *sulphate of copper* (CuO, SO_3), and the platinode cell with dilute sulphuric acid. The combination conducted well, 23·6 cubic inches of oxygen were collected from the zincode, and an equivalent of hydrogen from the platinode; but the platinode plate was scarcely covered with a very thin film of copper, the metal not having been transferred in any considerable quantity. Upon neutralizing the solutions on the two sides by carbonate of soda, it was found that an equivalent of sulphuric acid had travelled to the zincode. Upon reversing the arrangement, oxygen was collected as before from the zincode, and an equivalent of copper was thrown down upon the platinode, but no hydrogen was evolved.

(*z*.) The last experiments were varied by the substitution of solution of sulphate of potassa for the dilute sulphuric acid in combination with the sulphate of copper. When the copper solution was placed at the zincode, none of the metal passed over to the platinode, but a full equivalent of hydrogen was collected with alkali and acid at their respective electrodes. When the position of the two salts was reversed, the potassium travelled into the platinode cell, and was at once indicated by a precipitate of hydrated oxide of copper. The platinode plate was also covered partly with coherent copper, and partly with pulverulent metal, and only 3 cubic inches of hydrogen were collected for 71 cubic inches of mixed gases in the independent voltameter. None of the copper solution had penetrated to the zincode cell.

Thus it appeared that copper refused to exchange places with potassium in the circuit, as it had before refused to exchange with hydrogen, and so to pass from the zincode to the platinode.

(*a. a.*) *Sulphate of zinc* (ZnO, SO_3) was next subjected to the same treatment, and the results were exactly similar; the metal in all cases refusing like the copper to pass from the zincode division of the diaphragm cell to the platinode in any considerable quantity.

To ascertain whether the passage of the current from one electrolyte to another was in any way concerned in preventing the transfer of the base, we now resolved to employ *sulphate of copper* in solution *throughout* the whole arrangement, and having carefully ascertained the proportion of copper in solution, we placed 1500 grains of the liquid in each cell; metallic copper was thrown down on the platinode, and on precipitating the oxide from the liquid in the cell by potash, we found exactly the same amount of copper (taking the united quantities obtained from the oxide precipitated, and from the metal deposited) as before the experiment. An equivalent of sulphuric acid was set free at the zincode, but the quantity of the copper solution in the cell continued the same as at the commencement. Copper thus appeared to refuse to exchange with itself in electrolysis.

Sulphate of zinc afforded results exactly similar.

(*a. b.*) That the diaphragm itself did not throw any obstruction in the way of such transfer we satisfied ourselves, by varying its substance, from membranes to porous earthenware; but this was put out of all doubt in the following manner. A long siphon tube reversed was filled with a strong solution of sulphate of copper, and two strips of copper were made the electrodes of the battery; the action proceeded very slowly, but after a long time the liquid in the platinode limb of the apparatus became sensibly lighter-coloured, while that in the zincode became darker; the oxysulphion disengaged at the latter point had dissolved the copper of the zincode, which, however, had no power to pass to the platinode, to replace the metal which had there been deposited.

The preceding experiments bear closely upon an observation familiar to persons engaged in electrotyping on the large scale, viz. that the solution around the platinode becomes impoverished, and at last nearly deprived of copper.

As it appears to be thus established, that in the electrolysis of complex electrolytes, the metals which are capable of decomposing water at ordinary temperatures, or whose oxides are largely soluble in water, are susceptible of transference in the voltaic circuit from the zincode to the platinode, whilst those which are not marked by this strong affinity for oxygen remain stationary; we were anxious to ascertain to which class ammonium belongs.

(*a. c.*) The apparatus was charged with a solution of *muriate of ammonia* ($NH_4 Cl$) of known strength throughout; 35·5 cubic inches of hydrogen were collected from the platinode; the zincode liquid was yellow and smelt strongly of chlorine. By careful neutralization with muriatic acid, it was

ascertained that an equivalent quantity of ammonia had been disengaged in the platinode cell, and by precipitation of the chlorine with nitrate of silver, from the liquid thus neutralized, we found that the solution contained the same amount of chlorine as before the experiment; the amount of ammonium in combination with it, must, therefore, have remained un-changed: it had received no addition by transfer from the zincode side. The results of the experiment were perfectly clear and decisive.

(*a. d.*) In an experiment which was made in the same manner with *microcosmic salt* (NaO, $NH_4 O$, HO, $P_2 O_5$), the sodium travelled to the platinode, but the whole of the ammonium remained in the zincode cell. Here a *polybasic* acid was used, and yet results similar to those with a monobasic acid obtained. We have still further evidence that the nature of the acid does not appear to be connected with this travelling of the base, from the fact, that when the double *tartrate of soda and ammonia* $\left(\begin{matrix} NaO \\ NH_4 O \end{matrix} \right\} C_8 H_4 O_{10} \right)$ was electrolysed, putting a solution of soda into the platinode cell, the sodium was found to have travelled, but ammonium had refused to do so, as in the other cases already mentioned.

It was thus ascertained, contrary to the anticipations which we had formed upon its extreme solubility, that ammonium is included in that class of basic compounds which refuse to travel in the voltaic circuit.

(*a. e.*) To show that a relation exists between the power of transference in the circuit, and the affinity of the oxides of the metals, for water, the results of three experiments may be taken with salts of potassa, baryta and magnesia. For one equivalent of force as measured by 71 cubic inches of mixed gases from the voltameter,

14·2 grains of potassium $= \frac{1}{3}$ equivalent, were transferred from sulphate of potassa into sulphuric acid.

11·5 grains of barium $= \frac{1}{6}$ equivalent, were transferred from nitrate of baryta into a solution of the same salt.

1·0 grain of magnesium $= \frac{1}{12}$ equivalent, was transferred from sulphate of magnesia into sulphuric acid.

The experiments are not rigidly comparable together, nor must the quantities be taken as absolute determinations of the amount of each metal which travels in the circuit, but only as indicating a dependence of the transfer upon the attractive power of water or its elements.

The supposed anomalous transfer of sulphuric acid in the electrolysis of its mixture with water, the investigation of which cost so much labour upon a former occasion*, is now easily

* Second Letter, p. 222.

accounted for upon the hypothesis of the unequal transfer of the oxysulphion and hydrogen of the hydrate to their respective electrodes.

We were next induced to seek for some such connexion between the attraction of the different *acids* for water, and their transfer to the zincode. Our experiments had already embraced a great variety of acids, differing very much from one another in this respect, and in every case the transfer had taken place; but our attention had not yet been directed to the quantitative results.

(*a. f.*) As tungstic acid forms no definite hydrate, and is insoluble in water, we selected it as an extreme test of the fact, and for this purpose prepared some pure *tungstate of soda* (NaO, WO_3) by fusing together equivalent quantities of the acid and carbonate of soda. The product of the operation was dissolved and crystallized. A solution was made which contained one-twentieth of its weight of the salt; it was placed in a diaphragm apparatus, and was found to conduct badly. The current was transmitted for $1\frac{3}{4}$ hour, when the zincode liquid was found to be distinctly acid, owing to the formation of a soluble bitungstate, and the platinode very strongly alkaline. Upon precipitating the tungstic acid from both sides, by hydro-sulphuret of ammonia and nitric acid, it was decidedly shown that a portion of the acid had travelled to the zincode cell.

Neutral *chromate of potassa* (KO, CrO_3), which contains an acid forming no definite hydrate with water, furnished a perfectly analogous result; and the same thing was observed in the experiments with *arsenite of potassa*.

(*a. g.*) The carbonic acid is one of those which seems to have the least affinity for water, but it has been already shown that in the electrolysis of the carbonates of potassa, soda and ammonia, it is given off as oxy-carbion at the zincode in equivalent proportions. We now repeated the experiment. The diaphragm cell was charged throughout with *carbonate of soda* (NaO, CO_2) of definite strength, and the gas tube from the zincode cell connected with a drying apparatus, and Liebig's bulbs. The amount of carbonic acid was ascertained by weighing the bulbs, and by careful precipitation of the liquid in the cell by caustic ammonia and chloride of barium. By these means it was ascertained that there had been a decided transfer of the acid to the zincode, though in much less proportion than that of the sodium to the platinode. From the quantities of acid transferred in these experiments (which quantities however, as they can be considered but approximations to accuracy, we have not thought it necessary to record), it would appear that those acids which form no definite hy-

drates, are transferred to the zincode in smaller quantity than is the case with those from which distinct hydrates are obtainable.

(*a. h.*) We were extremely anxious to devise some means of ascertaining the phænomena of transfer in the case of simple electrolysis, but were forced to abandon the attempt after many trials, from the difficulty of procuring any substance which would answer the purpose of a permeable diaphragm, whilst it could resist the heat which is necessary to bring the chlorides, or similar binary compounds into the liquid state. We, however, ascertained that when a solution of *chloride of sodium* in water was electrolysed, the chlorine was transferred to the zincode in greater quantity than the sodium from it.

(*a. i.*) To remove the possibility of its being thought that water in these experiments conveyed the force independently of the presence of the saline bodies (a supposition hardly reconcilable with the phænomena observed where the saline solution was used throughout the apparatus), the following experiments were made. The platinode cell of the diaphragm apparatus was charged with dilute sulphuric acid, and the zincode with distilled water; one diaphragm alone was employed. The arrangement conducted so badly, that in an hour and a half, with twenty cells of the battery, only 1·5 cubic inch of hydrogen was collected from the platinode. The apparatus was then recharged and the electrodes reversed, the water being now at the platinode, and the acid at the zincode. In an hour and a half 2·8 cubic inches of hydrogen were collected from the platinode. From the imperfect conducting power evinced by water in these and other experiments, we can hardly suppose that the current, in cases where the base is not transferred, is carried by the electrolysis of this fluid, mysterious and ill-understood as its electrical relations undoubtedly are.

7. In reviewing the results which we have obtained from the preceding experiments, and the conclusions which we are entitled to draw from them, it will, we think, be admitted that many of them are of the highest interest and importance, and some of them at variance with the fundamental principles of electrolysis which have been hitherto admitted.

We have seen that in every instance the definite action of the electric current is maintained; and its passage through a compound liquid conductor is always marked by the disengagement at the platinode of hydrogen or the metallic element, or else of a group of substances, like ammonium, constituting an equivalent compound; and the simultaneous disengagement at the zincode of the non-metallic element, or a group

of substances of iso-electric powers. Of such electrolytes it may be convenient to distinguish the following classes:—

1st. An electrolyte may consist of *simple ions*, and then must be constituted of a single equivalent of a metal (or H) for its cation, and a single equivalent of a non-metallic element for its anion; as K, I; Ag, Cl; &c.; they may be termed *simple electrolytes*.

2nd. An electrolyte may consist of a *compound cation*, a single equivalent of which must take the place of a metal; and a single equivalent of a *simple non-metallic anion*, as NH_4, Cl. Organic alkalies probably form compound cations of this nature, and when their salts are electrolysed, hydrogen is always disengaged with them at the platinode, as with ammonia; these and the following we may call *complex electrolytes*.

3rd. An electrolyte may consist of a *compound anion*, a single equivalent of which would take the place of the simple non-metallic anion, with a single equivalent of a *simple cation*, a metal (or H), as H, NC_2; K, SO_4; Na, NO_6.

4th. An electrolyte may consist of a single equivalent of a *compound cation*, and a single equivalent of a *compound anion*, as NH_4, SO_4.

These four cases may be included in the term *Monobasic Electrolytes*, as a single equivalent of force (measured by the voltameter) would electrolyse single equivalents of the electrolytes.

5th. An electrolyte may also consist of *two or more equivalents of a metallic cation* or (H), or of *single equivalents of two or more metallic cations* (or H); when the *anion* must consist of a *single equivalent of a compound ion*, as $(K_2 \, FeCy_3)$. This compound ion, in the case of an oxysalt, contains the so-called anhydrous acid in combination with as many equivalents of oxygen as there are of metallic cations (or H) in the compounds, as $(Na_3, \, \overline{P_2 O_5, \, O_3})$.

In this case as many equivalents of force will be required for the electrolysis of one equivalent of the electrolyte as there are equivalents of metal (or H) in the cation. They may be denominated *Polybasic Electrolytes*.

In these compound anions and cations, it would appear that the oxygen which travels with the acid group, and the hydrogen which is evolved with the alkaline group, must be connected with the other elements whilst under the influence of the current in a manner differing from that in which the latter are combined together; for we have found that in most cases this connection is immediately dissolved upon their escape from the electric influence, whilst in some others their apparent permanent combination is only the effect of secondary

action, where the oxygen is capable of forming a chemical compound of a higher degree of oxygenation, and like other secondary actions of a similar nature, is variable in its amount.

8. The disengagement of the cation and anion of an electrolyte in equivalent proportions is not always effected, as is commonly represented, by their simultaneous transfer in opposite directions to their respective electrodes, in the exact proportion of half an equivalent of each; but is sometimes brought about by the transfer of a whole equivalent of the anion to the zincode, whereby a whole equivalent of the cation is left uncombined at the platinode; or by transfer of unequivalent portions of each in opposite directions, making however together a whole equivalent of matter transferred to one electrode or the other; or speaking more correctly, *by the transfer of a quantity of matter capable of exerting one equivalent of chemical force,* so that when the anion *transferred* to the zincode exceeds half an equivalent, the cation transferred to the platinode is in an equal proportion less than half an equivalent, and *vice versâ*; the anion and cation *set free* being always in equivalent proportions. We have, however, in no case observed the transfer of a whole equivalent of the cation to the exclusion of the anion.

These facts are, we believe, irreconcilable with any of the molecular hypotheses which have hitherto been imagined to account for the phænomena of electrolysis, nor have we anything more satisfactory at present to substitute for them; we shall therefore prefer leaving them to the elucidation of further investigation, to adding one more to the already too numerous list of hasty generalizations.

XLI. *On an improved Method for the Detection and Quantitative Determination of Arsenic.* By REMIGIUS FRESENIUS, *M.D.*[*]

THE greater part of this paper is occupied with the discussion of the medico-legal questions arising in cases of poisoning; the author advocates the interference of the legislature to render it imperative on chemists to pursue a given method in seeking for the presence of poisons: and after pointing out the sources of uncertainty in all the methods employed hitherto for the detection of arsenic, he proposes a new one, which he alleges is free from every objection, which method is the result of many investigations pursued by himself in conjunction with Dr. von Babo.

[*] Communicated by the Chemical Society; having been read April 15, 1844.

Dr. Fresenius states the objection to the method of Reinsch to be :—1. That it does not admit of the detection of arsenic in every form in which it may exist. 2. It does not aid the detection of other poisons. 3. It contaminates the substances under examination with copper. 4. Its success in precipitating the arsenic is impeded or altogether prevented by the presence of many substances, nitrates, mercury and other metallic compounds. 5. Almost insurmountable difficulties in determining the arsenic quantitatively are attached to this method.

The method of Marsh presents these difficulties:—1. It does not admit of the separation of the arsenic under every form in which it may occur. 2. It contributes to the contamination of the substance under examination with zinc and other metals. 3. The danger of mistaking antimony mirrors for arsenic. 4. Mirrors are either not formed at all, or their brilliancy is prevented, and the proof of the existence of arsenic rendered doubtful by the presence of water, of organic substances, and atmospheric air.

The method of investigating the body in cases of suspected poisoning, for the presence of arsenic, which we propose, is as follows :—

A. *Decoloration and Solution.*

The substances intended for examination are, if they consist of solid and coherent lumps, reduced into small pieces, and under all circumstances carefully intermixed. Two-thirds of the whole mixture are then put into a large porcelain basin, the remainder is kept in a well-closed vessel for unforeseen contingencies. The mixture in the porcelain basin is then drenched with an amount of fine concentrated hydrochloric acid, either equal or superior to the weight of the dry substance it contains, and with as much water as will give the consistence of a thin pap to the whole. The basin is then heated in the water-bath, and chlorate of potash in portions of about half a drachm added to the mixture at intervals of about five minutes, and until the contents of the basin has assumed a bright yellow, perfectly homogeneous and thin liquid appearance. When this point is attained, about 2 drachms more of chlorate of potash is added to the mixture, and the basin is removed from the water-bath. When completely cool, the contents are placed upon white filtering paper or upon a linen filtering cloth, and the fluid allowed to run off: the residue is washed with hot water until the liquid running off no longer manifests any acid reaction : the water used in this washing operation is added to the filtrate. The

whole liquid thus obtained is then concentrated in the water-bath to the weight of 1 pound (during their operation the bright yellow colour of the fluid in most cases changes into a brownish tint), and a saturated solution of sulphurous acid in water is added to the residuary, still very acid, fluid, with stirring, until the smell of the sulphurous acid becomes clearly perceptible. The whole mixture is then again heated for about an hour, until the excess of sulphurous acid is completely expelled.

B. *Separation of the Arsenic as Sulphuret of Arsenic.*

The fluid thus obtained (and which is about double the amount of the hydrochloric acid employed) is allowed to cool, and then poured into a beaker glass, and a slow stream of washed sulphuretted hydrogen gas transmitted through it for the space of about twelve hours; the gas conducting tube is rinsed with some solution of ammonia, and the ammoniacal solution thus obtained added to the fluid in the beaker glass; this glass is then lightly covered with printing paper and kept at a very moderately warm heat (30° C.) until the smell of sulphuretted hydrogen has completely disappeared. The precipitate obtained in this manner is collected upon a filter, which ought not to be too large, and washed.

C. *Purification of Sulphuret of Arsenic so obtained.*

The precipitate obtained in B, and which besides organic matter, contains, as sulphuret of arsenic, all the arsenic originally present in the substance under investigation, and possibly moreover other metallic sulphurets, is then, together with the filter, dried in a small porcelain basin, heated in the water-bath: fuming nitric acid is added drop by drop until the whole is moistened; the moist mass is then evaporated to dryness in the water-bath. Pure, previously heated, hydrated sulphuric acid is then added to the residue, so as to moisten it uniformly; the mass is heated in a water-bath for the space of from two to three hours, and finally in the sand-bath at a somewhat higher but still moderate temperature (150° C.), until the charred mass begins to crumble. The residue is treated with from 10 to 20 parts of distilled water, until the last drops running off no longer manifest any acid reaction: the water employed in the washing is added to the filtrate.

D. *Quantitative Determination of the Sulphuret of Arsenic.*

The limpid fluid obtained in C is mixed with some hydrochloric acid and precipitated by sulphuretted hydrogen, as directed in B. The precipitate, after the supernatant fluid has

completely lost the smell of sulphuretted hydrogen, is filtered upon the smallest possible filter, carefully washed, and while still moist upon the filter drenched with solution of ammonia, and the filter also washed with dilute ammonia until perfectly free from the precipitate. The ammoniacal fluid is then poured into a small porcelain basin, the weight of which has previously been exactly determined, and evaporated in the water-bath; the residue is dried at $100°$ C. and weighed. Should the subsequent reduction prove that the residue consisted of arsenic alone, then 0·803 is the amount of arsenious acid to be reckoned for every 1000 of the sulphuret of arsenic obtained. Should a residue insoluble in ammonia have remained upon the filter, this must be tested for lead, bismuth, copper, &c. &c. Lead and mercury may moreover be present in the charred residue of C; this latter also must therefore be further tested for the presence of these substances.

E. *Reduction of the Sulphuret of Arsenic.*

The separation of the metallic arsenic from the sulphuret of arsenic, as the key-stone of the whole investigation, requires the greatest care and attention. The apparatus sketched in fig. 1 is used for the reduction of the sulphuret of arsenic.

Fig. 1.
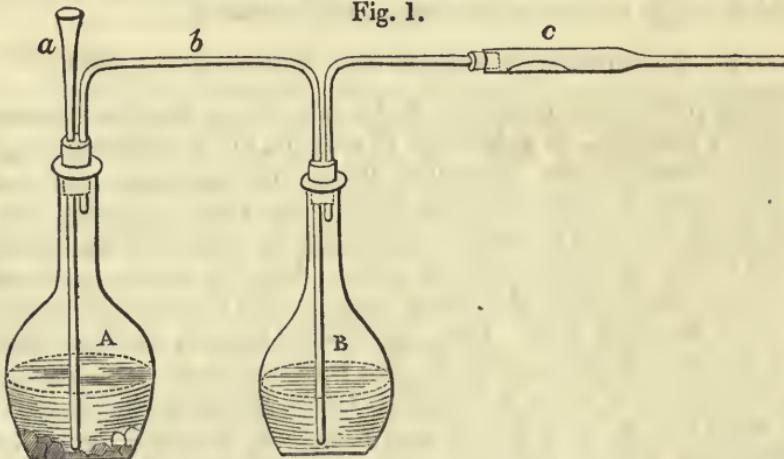
A is a capacious flask for the evolution of carbonic acid. It is filled to half its size with rather large pieces of solid lime-stone or marble (not chalk, since this last substance will not yield a continuous stream). To one aperture of the doubly-perforated cork a funnel tube *a* is adapted, which nearly reaches to the bottom of the vessel; to the other aperture a tube *b*, by means of which the gas evolved is conducted into a flask of smaller size, B; in this flask it is washed and dried by the hydrated sulphuric acid contained therein. The tube

c conducts the carbonic acid into the reduction-tube C (which is sketched in fig. 2 of its natural size). This reduction-tube must be made of difficultly fusible glass.

When the apparatus is prepared, the little basin with the sulphuret of arsenic (vide D.) is again weighed, one-third of its contents taken off and put upon a watch-glass; the exact weight of this abstracted part is determined by substituting weights for it. The basin containing the two-thirds remaining is carefully kept. The abstracted third intended for reduction is rubbed in a small basin, previously heated in the water-bath, together with about 12 parts of a mixture consisting of 3 parts of dry carbonate of soda and 1 part of cyanide of potassium (prepared according to Liebig's method); the completely intermixed powder is put upon a small strip of card-paper bent into the shape of a gutter. This is put into the reduction-tube up to the point *f*, and the tube is then turned half round its axis.

In this manner the mixture is placed at *a* of the reduction-tube without soiling any other part of the tube, the strip of card paper is then cautiously withdrawn from the tube, so as to leave the mixture undisturbed. The reduction-tube is then, by means of the cork *e*, fixed to the gas-evolution apparatus; a moderate stream of carbonic acid gas is evolved by pouring hydrochloric acid into the funnel tube (*a*, fig. 1), and the mixture most carefully dried by very moderately heating the tube along its whole length by means of a small spirit-lamp. When every trace of water has disappeared from the tube, and the gas stream has become so slow that the bubbles pass through the sul-

S 2
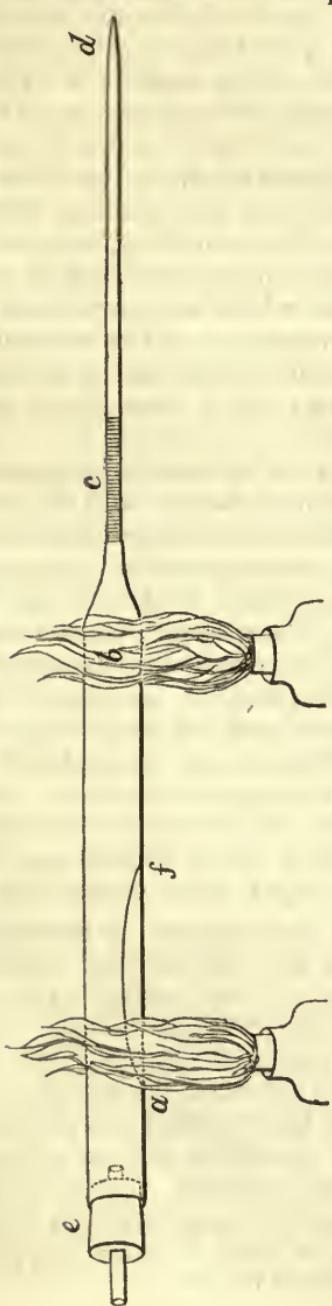
phuric acid at intervals of about a second, the spot b is heated to redness by means of a spirit-lamp. When this point is attained, another strong spirit flame is applied to the mixture progressing from a to f, until all arsenic is expelled (the first flame continues at the same time acting at b): the reduced arsenic precipitates at the spot c forming a mirror, whilst an exceedingly small portion escapes at d and fills the air with the smell of garlic. The second spirit-lamp is at last slowly advanced to the spot b, so as to drive towards c all the arsenic which has adhered to the walls of the wide tube. After this, both lamps are removed, the tube closed at the point d by fusion, and heat applied progressively from the point d towards c, so as to contract the mirror on this side also, which causes it to exhibit a particularly fine and metallic appearance; the tube is then cut off at the point f, closed, sealed, and thus becomes a permanent evidence, which may be referred to in any future point of the legal proceedings. The remaining two-thirds of the sulphuret of arsenic (vide *supra*) are also put into a small glass tube, and this too is then closed and sealed and preserved for reference.

If sulphuret of zinc or sulphuret of antimony be present, together with the sulphuret of arsenic, the zinc and the antimony will be obtained in their metallic state upon dissolving in water the residue found in the reduction-tube (antimony is also found in the solution); they must be determined by the ordinary methods now in use. Their weight is calculated upon the whole amount of the residue originally contained in the small porcelain basin (vide D), and the weight of their corresponding sulphates subtracted from the total weight of the residue. The remainder is the amount of sulphuret of arsenic corresponding to the arsenic present.

XLII. *On Fluorine in Recent and Fossil Bones, and the sources from whence it is derived. By* J. MIDDLETON, *Esq.*[*]

HAVING been for some time past engaged in investigations, not yet matured, on the absolute and relative quantities of fluorine in fossil bones, I was readily led to inquire into its presence, or otherwise, in recent bones. The high authority of Berzelius had indeed satisfied me on this subject, and I might not have felt a motive to examination for myself had I not lately heard the fact doubted and disputed before the Chemical Society and elsewhere with an earnestness which could only proceed from conviction. The readiness

[*] Communicated by the Chemical Society; having been read May 6, 1844: on the subject of this paper, see the author's previous one, p. 14, and also Dr. Daubeny's, p. 122 of the present volume.

with which the authorities of the University College acceded to my request for materials, as well for this as for my more laborious investigations, left me no difficulty, and deserves my best acknowledgements.

I easily obtained conclusive evidence of the presence of fluorine in the following portions of the human skeleton, the bones operated upon being from the dissecting-room :—The *occiput*, the *vertebræ*, the *humerus*, the *femur*, the *teeth*, the femur of a fœtus of $6\frac{1}{2}$ months.

I examined also the arm, including the scapula, of a fœtus of $3\frac{1}{2}$ months, but could obtain no evidence of the presence of fluorine in it, a result which, considering the small quantity of osseous matter involved, was, perhaps, to have been looked for.

I determined also the presence of fluorine in the entosternal bones of the sternum of a recent tortoise.

Any one who may continue to entertain doubts on this subject, and whose object is the recognition and discovery of truth, may readily convince himself by using the means employed by me. I broke a portion of the bones to be examined into small fragments, and subjected them to the action of concentrated sulphuric acid in a platinum crucible covered, as is usual in such operations, by a plate of glass endued with an etched coating of wax. I applied the flame of a spirit-lamp from time to time, so moderating the heat as to sustain action of the acid upon the materials without projection upward of the substances against the glass. I prevent the melting of the wax by keeping a muslin rag moist with alcohol upon its upper surface. The time occupied by each experiment was between five and ten minutes.

Through these and other investigations above alluded to, I have ascertained the presence of fluorine in the organic remains of Carnivora, Herbivora, Reptilia, Pisces, as also in the recent bones of men and reptiles. The increase of fluorine in fossil bones is apparently greater in proportion to the remoteness of the period at which they lived, where the character of entombment is similar. These facts, taken conjointly, seemed to me to need for their explanation a more general source of fluorine than has been heretofore I believe supposed. It occurred to me that ordinary water might be the vehicle, and if so, the presence of fluorine in recent bones would not only be accounted for, but also its accumulation in fossil bones, being filtered from the moisture circulating in the earth's crust. In order to ascertain whether facts would be found to sustain this view, I examined the following substances :—

First. A deposit, chiefly of sulphate of lime, from, as it appeared, a chloride of calcium vat, and found it to contain fluorine, though in small quantity. As it was suggested to me, however, that glass retorts used for the distillation of hydrochloric acid have been known to be thereby corroded, I did not attach much weight to the result, although I drew encouragement from it.

Second. A deposit formed in a wooden conduit pipe in a coal mine, procured for me by my friend Dr. Falconer, and found it to contain a still greater proportion of fluorine than the former.

Third. A stalactitic deposit, said to have been formed in an aqueduct in France. It was of a pure white colour, and made up of very thin and scaly concentric layers, being at the same time very incompact : it contained no fluorine.

Fourth. A stalactitic deposit from a cave in old red sandstone, furnished to me by Mr. Arrott, to whom for this and for other assistance in my investigations I am much indebted : this I found to contain fluoride of calcium to the extent of about 9 per cent. The stalactite consisted chiefly of carbonate of lime.

Fifth. The crust formed on the inside of a kettle used for the boiling of water : this I found to afford faint but distinct proof of the presence of fluorine.

Lastly. A fragment of a vein of sulphate of barytes found in the sandstone above mentioned. This also contained fluorine, though in much less proportion than the stalactite of the fourth experiment.

The above are the only substances, sufficiently diverse in their origin, which I have had an opportunity of examining, and the facts I have elicited from them seem to confirm the justness of my theory of the prime sources of fluorine in bones. It follows, as a necessary corollary, that it exists in most if not all vegetables, though perhaps in minuteness of quantity, that may enable it often to elude detection.

XLIII. *On the occurrence of Mannite in the* Laminaria saccharina *and other Sea-weeds.* *By* JOHN STENHOUSE, *Ph.D.*[*]

MORE than thirty years ago the *Laminaria saccharina*, with some others of our commonest sea-weeds, were examined by Vauquelin, who detected in several of them a sweet-tasted

[*] Communicated by the Chemical Society; having been read May 6, 1844.

crystalline substance. Subsequently, in 1815, the *L. saccharina* and five other fuci were again subjected to analysis by M. Gaultier de Claubry, who also detected in the *L. saccharina* and in the *Halydris siliquosa* the same saccharine substance, which he called manna.

From the imperfect state of organic chemistry at these early periods, neither of these eminent chemists were enabled to subject this saccharine substance to analysis; and so little reliance has been placed upon their statements, that for some years past they have been generally discredited both by chemists and botanists; Berzelius, for instance, has not once mentioned sea-weeds as a source of mannite, though he has enumerated all the other known sources; and Dr. Greville, in his *Algæ Britannicæ,* regards the crystalline incrustations which usually appear on the *L. saccharina* and the *Halydris siliquosa* when dried, as nothing else than common salt. Under these circumstances I was lately induced to subject the *L. saccharina* and some other sea-weeds to examination.

A quantity of the dried *L. saccharina* was repeatedly digested with hot water, which formed with it a mucilaginous solution of a brownish red colour, and of a sweetish but at the same time of a very disagreeable taste. When evaporated to dryness on the water-bath, it left a considerable quantity of a saline semi-crystalline mass. This was reduced to powder and treated with boiling alcohol, by which a considerable portion of it was dissolved. The alcoholic solution on cooling became nearly solid, from the quantity of long transparent prismatic crystals with which it was filled. When purified by a second crystallization, these were deposited in large hard prisms of a fine silky lustre, and possessing all the external characters of mannite. When dried at 212° F., they were subjected to analysis.

I. 0·4334 grm. of substance gave 0·6235 of carbonic acid and 0·2995 of water.

II. 0·4215 grm. gave 0·6058 carbonic acid and 0·294 water.

	1.	II.	Calculated Numbers. At.	Per cent.
Carbon	39·78	39·74	6 Carbon	40·02
Hydrogen	7·67	7·74	7 Hydrogen	7·62
Oxygen	52·25	52·52	6 Oxygen	52·36
	100·00	100·00		100·00

It is evident from the result of these analyses, that the sweet crystalline substance in the *L. saccharina* is nothing else than mannite. The quantity of mannite contained in it is very consi-

derable. One thousand grains of the dried sea-weed, when treated in the way already described, first with water and then with alcohol, gave 121·5 grains = 12·15 per cent. The specimen operated on had been kept for six months, and was covered with an incrustation in long slender needles. I also examined a quantity of the fresh sea-weed, and found it to contain as much mannite as the older specimen. It appears, therefore, that the epithet *saccharina*, as applied to this species of Laminaria, is not so inappropriate as has been often supposed.

Mannite may be easily distinguished from cane-sugar by the following test. If a little strong sulphuric acid is poured upon the mannite and a gentle heat is applied, the mannite dissolves without being in the least discoloured, and gives a transparent solution. If the heat is much increased the liquid becomes of a deep brown colour, but does not lose its transparency. When cane-sugar, on the contrary, is gently heated with sulphuric acid, it is, as is well known, immediately charred with evolution of sulphurous acid gas. From grape-sugar mannite may be likewise easily distinguished. If mannite is boiled with a strong solution of potash or soda, it dissolves without any change of colour, while grape-sugar when similarly treated acquires a deep brown colour. When heated with a solution of potash and some sulphate of copper, mannite completely prevents the precipitation of the oxide of copper, while grape-sugar causes the immediate precipitation of the red oxide of copper.

Besides mannite, the *L. saccharina*, in common with most of the other sea-weeds, contains a great deal of a peculiar mucilage, which when dried has a deep reddish colour. It differs however from ordinary gum, for when digested with nitric acid it yields oxalic, but neither mucic or saccharic acids. I intend subjecting this substance to more minute examination.

L. digitata.—Besides the *L. saccharina* I have also examined some of the other sea-weeds for mannite, and among others the *L. digitata* or common tangle. The aqueous solution of this sea-weed is also reddish brown, and when evaporated it yields a similar mucilage with the *L. saccharina*, but in much smaller quantity. The *L. digitata* also contains a considerable quantity of mannite, though I should think scarcely half as much as what exists in the *L. saccharina*.

Halydris siliquosa.—The next sea-weed examined was the *Halydris siliquosa*. With hot water it forms a very dark-coloured solution of a bitter and slightly astringent taste. The quantity of mannite contained in it is very great, amounting I should think to between 5 and 6 per cent. As already men-

tioned, mannite forms a great part of the white incrustations which appear on the surface of this sea-weed when dried.

Alaria esculenta.—This beautiful sea-weed, which is by no means uncommon on the coasts of Scotland, where, as its name imports, it often serves as an article of food, also contains mannite in considerable abundance.

Rhodomenia palmata.—*Rhodomenia palmata,* or common dulse, contains a good deal of a sweet-tasted greenish-coloured mucilage. It also yields a considerable quantity of mannite, amounting probably to 2 or 3 per cent.

Fucus vesiculosus.—The *Fucus vesiculosus,* the most common perhaps of British Algæ, contains I should think from 1 to 2 per cent. of mannite, and the *Fucus nodosus,* also a very common sea-weed, likewise yields a small but very appreciable quantity of the same principle.

Fucus serratus.—This sea-weed also contains a considerable quantity of mannite, less perhaps than the *L. digitata,* but more than the *Rhodomenia palmata.* The mannite which the *Fucus serratus* yields is much freer from colouring matter than that from any of the other Algæ, being nearly colourless from the first.

I could not detect any mannite in the *Ulva latissima* or Laver. The experiment was made on a very small scale, and will be repeated on the first opportunity. The Laver contains a good deal of a sweet-tasted green-coloured mucilage, similar to that of the *Rhodomenia palmata.*

As mannite has occurred in eight out of nine of the sea-weeds which I have happened to examine, it probably exists in larger or smaller quantity in most sea-weeds, in which it appears to replace the cane- and grape-sugar so abundant in many of our land-plants. It is evident also that mannite occurs much more plentifully in nature than has been hitherto imagined. The following is a list of the Algæ just described, arranged in order according to the quantity of mannite which they severally contain :—

1. Laminaria saccharina.	5. Alaria esculenta.
2. Halydris siliquosa.	6. Rhodomenia palmata.
3. Laminaria digitata.	7. Fucus vesiculosus.
4. Fucus serratus.	8. Fucus nodosus.

The quantity of mannite in the *L. saccharina* is such that I think mannite might be more œconomically procured from this sea-weed than from the usual source, manna.

Supposed existence of Mannite in the Roots of Triticum repens, or Couch-grass.

In a note on the article Mannite, in the eighth volume of the last German edition of Berzelius's *Lehrbuch*, it is stated that Professor Pfaff had succeeded in obtaining mannite from the roots of *Triticum repens*, or couch-grass. M. Pfaff says that he treated the extract of the couch-grass roots with boiling alcohol, which on cooling deposited a number of long needle-shaped crystals, which he believed to be a new species of sugar, but which Berzelius is rather disposed to regard as more probably mannite. I have twice repeated M. Pfaff's experiment on two different quantities of couch-grass roots, but with very opposite results. The alcoholic solution on standing deposited it is true a quantity of long slender crystalline needles. These, however, had not a sweet taste, and when thrown into hot sulphuric acid they dissolved with effervescence without blackening the solution. When heated on platinum foil they melted, and left a white fusible alkaline residue, which, when neutralized with muriatic acid, produced a yellow crystalline precipitate in an alcoholic solution of platinum. I have every reason to believe, therefore, that these acicular crystals were merely binoxalate of potash. The grass roots certainly contained a great deal of an uncrystallizable sugar which readily fermented.

XLIV. *First Memoir on various Phænomena of Induction. By* M. Elias Wartmann, *Professor in the Academy of Lausanne*[*].

1. THE phænomena of electric induction have been studied for some years past by a great number of physicists. Much however is still to be desired in the way of the determination of the laws which govern them and the establishment of a theory which binds all these laws together.

2. In this first memoir I propose to make known various new results which I have obtained. I shall hereafter endeavour to show what is their relation to other electric phæno-

mena, whether unpublished or already known, by endeavouring to sketch out this general theory which the science does not possess.

§ I. *Description of the Apparatus.*

3. I constructed a triple helix of large dimensions by rolling at one operation, on a great wooden bobbin, three copper wires covered with silk and perfectly annealed. Each of these wires is $23^{m}\cdot6$ long and $0^{m}\cdot003$ in diameter. They are so arranged that in all the circumvolutions the middle wire preserves its position with regard to the two others. It is this which, for brevity, I shall designate the *induced wire*, reserving the name of *inductor wires* for its two neighbours indifferently. Their diameter is sufficient to ensure that they are never heated by the currents to which they are subjected. An aperture is formed in the bobbin, destined in certain cases to receive a cylinder of soft iron of $0^{m}\cdot17$ long by $0^{m}\cdot05$ in diameter.

4. A small helix was also formed of three copper wires, not more than $0^{m}\cdot0008$ in diameter. Two amongst them are equal, and each of them makes 500 turns; the third, a little thicker, only makes 75 revolutions on the rectangular frame of wood which forms the interior of the apparatus: a parallelopiped of soft iron may be introduced into this frame. This helix differs from the first, in the circumstance that the wires in their superposed layers are always in the same direction and do not cross each other.

5. The measuring instruments which were used are the following:—

a. A multiplying rheometer of 3000 turns, to which an almost astatic system of very light needles gives extreme sensibility; I shall call it the *hydro-electric rheometer.*

b. A second rheometer, likewise very delicate, and which I shall call the *thermo-electric rheometer*, because its wire is thicker and only forms 75 revolutions.

c. A *metallic thermometer* of Bréguet, the helix of which, composed of silver, of gold, and of platinum, presents 46 free spiral turns. It is arranged so that it may be placed in a voltaic circuit. The instrument appreciates $0°\cdot0714$ cent.*

d. A *needle* called *astatic*, the graduated circle of which is $0^{m}\cdot102$ in diameter: metallic wires of different dimensions might in this instrument be stretched in the plane of the magnetic meridian, and they would thus become parallel to the axis of the needle.

* For a complete description of this instrument, see *Memoires de la Société de Physique et d'Histoire Naturelle de Genève*, vol. ix. p. 123, or *Archives de l'Electricité*, vol. i. p. 77.

e. A small *helix for magnetizing*, formed of a wire of plated copper (*cuivre argenté*) of 0^m·00065 in diameter, making 94 revolutions around a tube of reed.

6. The pile employed is constructed of separate elements and has a constant force. The zincs are amalgamated solid cylinders, of 0^m·15 long and 0^m·045 in diameter; they are immersed in a solution of chloride of sodium, inclosed in membrane. The coppers are hollow cylinders of 0^m·06 in diameter, immersed in a saturated solution of sulphate of copper. The energy of this apparatus is sustained during more than five hours without very perceptible variations[*].

7. The *additional wires* of which we shall treat were all carefully annealed; they are of the following dimensions. (See the annexed Table.)

Nature of the wires.	Lengths.	Diameters.
	metres.	metres.
Copper	9·510	0·00072
Platinum	0·435	0·00033
Brass, No. 1 ...	9·760	0·00230
Brass, No. 2 ...	15·640	0·00025
Iron, No. 1 ...	1·830	0·00034
Iron, No. 2 ...	6·743	0·00150
Iron, No. 3 ...	22·128	0·00020

8. In all the experiments related in this memoir we shall only treat of currents induced by the completion of the voltaic circuit, unless the contrary be expressly indicated.

9. The numbers stated in the tables are the mean of several readings according well with each other. To obtain this mean was essential, whether as a guarantee against the variations of intensity of the pile, or especially on account of the difficulty of the readings when the needles, yielding to an instantaneous deviating action, traverse an arc, the precise amplitude of which it is necessary to appreciate.

10. The needles of the rheometers generally coincided with the seventh degree to the right or left of the zero of graduation; when they were in equilibrium, having no compass with sines at my disposal, I strove to leave to the most intense currents an energy which would cause the index to deviate not more than from thirty to forty degrees, so that I might consider the angle described as sensibly proportional to the force which acted on the needle.

§ II. *Induction of a constant wire by a variable wire.*

11. The circuit of the induced wire of the great helix with

the thermo-electric rheometer was closed. That of one of the inductor wires was also closed by plunging its two extremities into a capsule full of mercury. Lastly, the other inductor wire was put into communication with a pile of two elements, either directly or by the intermediation of the brass wire No. 2, the length of which had been made variable.

12. Repeated experiments have shown, that for the additional lengths of wire increasing in geometrical progression, the intensities of the induced current, measured by the rheometer, diminish in arithmetical progression (α).

13. The same trials have been repeated by *opening* the circuit of the second inductor wire, all other circumstances remaining constant. Their results have been as follows:—

a. For the additional lengths of wire increasing in geometrical progression, the intensities of the induced current, measured by the rheometer, again diminish in arithmetical progression (β).

b. The intensity of the induced current measured by the rheometer is greater when the circuit of the second inductor is closed than when it is open (γ).

c. From the existence of the laws (α) and (β) it results that for the lengths of additional wire increasing in geometrical progression, the differences of intensity of the induced current, measured by the rheometer, when the circuit of the second inductor wire is closed and when it is open, decrease according to an arithmetical progression (δ).

14. Let M be the intensity of the induced current measured by the rheometer (general term of the arithmetical progression);

T the corresponding term of the geometrical progression;

a the first term of the arithmetical progression;

e the first term of the geometrical progression (unit of the additional length of wire);

r ratio of arithmetical progression;

q ratio of geometrical progression.

These quantities are connected by the relations

$$M = a - r \left(\frac{\log T - \log e}{\log q} \right),$$
$$T = e\, q \left(\frac{a - M}{r} \right).$$

15. The following Tables will serve as a proof of these enunciations. The ratio of arithmetical progression has been designated by *r*.

Lengths of additional wire. $q=2$.	Second inductor wire closed.			Second inductor wire open.		
	Deviations of the rheometer.		Differences. Mean error $=0°·75$.	Deviations of the rheometer.		Differences. Mean error $=0°·21$.
	Observed.	Calculated. $r=2°·33$.		Observed.	Calculated. $r=1°·50$.	
1	19·00	19·00	0·00	14·00	14·00	0·00
2	19·00	16·66	—2·34	15·00*	12·50	—2·50
4	16·50	14·33	—2·17	11·00	11·00	0·00
8	13·00	12·00	—1·00	9·00	9·50	+0·50
16	10·00	9·66	—0·34	7·00	8·00	+1·00
32	7·00	7·33	+0·67	7·00	6·50	—0·50
64	5·00	5·00	0·00	5·00	5·00	0·00

Calculated deviations of the rheometer.				
Additional lengths. $q=2$.	Second inductor wire.		Excess.	Constant differences.
	Closed.	Open.		
1	19·00	14·00	5·00	0·84
2	16·66	12·50	4·16	0·83
4	14·33	11·00	3·33	0·83
8	12·00	9·50	2·50	0·84
16	9·66	8·00	1·66	0·83
32	7·33	6·50	0·83	0·83
64	5·00	5·00	0·00	

16. From the preceding logarithms we should conclude, that when the conducting wire of a current is connected with another wire convolved in the form of a helix, its conductibility measured by the intensity of the current which it is capable of inducing, varies for different portions of its length according to a different law from that which it follows when it only completes the circuit, admitting proportionality to exist between the inducing and the induced current, as several physicists suppose (ε).

17. In order to verify this conjecture, the copper wire was placed parallel to the axis of the astatic needle, situated in the plane of the magnetic meridian, and the *maximum* amplitude of the arc traversed by this needle was observed, as well as the value of its stable deviation, in the following different alternatives (the additional copper wire being united with the first inductor wire to close the circuit of a pile of two elements) :—

a. The circuit of the second inductor and that of the induced wire both being closed.

b. These two circuits being open.

c. The one being shut, the other open.

* This abnormal result is either an error of reading of the assistant, occasioned by the smallness of the interval between the divisions of the dial, or a heating of the wire under the influence of the current.

d. The circuit of the induced wire being closed by a pile of a single pair, and that of the second inductor being open.

e. Lastly, the same conditions existing as in the case *d*, but the circuit of the second inductor being closed by some mercury.

18. These experiments have led to the following results:—

a. The state of the simultaneous or separate closing or opening of the circuits of the induced wire and of the second inductor wire has no influence on the intensity of the inducing current (ζ).

b. This intensity diminishes in arithmetical progression, for the lengths of the inductor wire increase in geometrical progression (η).

c. The intensity of the inducing current is independent of the presence or the absence of a voltaic current in the induced wire; it does not vary with the state of closure or of opening of the circuit of the second inductor (θ).

19. The annexed Tables contain the confirmation of these laws.

Lengths of additional wire. $q=2.$	Readings of the astatic needle.			Differences. Mean error $=0°·90.$
	Impulsions.	Deviations.		
		Observed.	Calculated. $r=2°·90.$	
1	100·00	34·50	34·50	0·00
2	90·00	31·50	31·60	+0·10
4	80·00	31·00	28·70	—2·30
8	70·00	28·00	25·80	—2·20
16	60·00	24·00	22·90	—1·10
32	44·00	20·00	20·00	0·00

Intensities of the inductor wire measured by the astatic needle, amplitudes and deviations.

Lengths of additional wire. $q=2.$	Second inductor wire, closed.			Second inductor wire, open.			Second inductor wire, closed or open.		
	Amplitudes.		Differences. Mean error $=0°·76.$	Amplitudes.		Differences. Mean error $=2°·57.$	Deviations.		Differences. Mean error $=0°·97.$
	Observed.	Calculated. $r=12°·25.$		Observed.	Calculated. $r=11°·50.$		Observed.	Calculated. $r=3°·00.$	
1	117·50	117·50	0·00	113·00	113·00	0·00	38·00	38·00	0·00
2	105·00	105·25	+0·25	104·50	101·50	—3·00	35·30	35·00	—0·30
4	99·00	93·00	—6·00	99·50	90·00	—9·50	34·50	32·00	—2·50
8	79·50	80·75	+1·15	81·00	78·50	—2·50	30·00	29·00	—1·00
16	70·00	68·50	—1·50	70·00	67·00	—3·00	28·00	26·00	—2·00
32	55·50	56·25	+0·76	55·50	55·50	0·00	24·00	23·00	—1·00
64	44·00	44·00	0·00	44·00	44·00	0·00	20·00	20·00	0·00

20. In order to unite all the desirable demonstrations of the exactness of these results, I have repeated with the same wire and under similar circumstances, some experiments on its conductibility as a function of its length, the wire and the thermo-electric rheometer being alone in the circuit. I obtained, as it was easy to foresee, values which agree with the formula of M. Ohm*. Thus the remark (16.) is established.

§ III. *Induction of a constant wire by two wires, one of which is variable.*

21. Proceeding from the facts above related, similar results for the simultaneous induction of two inductor currents on a wire placed symmetrically between them, were to be expected. The subject divided itself naturally into six cases, which were examined. Calling the currents induced at the time of the closing of the circuit *direct currents*, those which are induced at the time of its rupture *inverse currents*, these six cases are characterized as follows :—

a. The two inductors are equal and traversed by direct currents in the same direction.

b. The two inductors are equal and traversed by direct currents in contrary directions.

c. The two inductors are unequal and traversed by direct currents in contrary directions.

d. The two inductors are unequal and traversed by direct currents in contrary directions.

e. The two inductors are equal and traversed, one by a direct current, the other simultaneously by an inverse current.

f. The two inductors are unequal and traversed, one by a direct current, the other by an inverse simultaneous current.

22. *First case.*—Different precautions are requisite in the arrangement of the apparatus, in order not to bring any perturbating force into play. I found that the current of ten pairs, in passing through the two wires of the small helix, heated them rapidly. On the other hand, the great conductibility of the wires of the great helix causing each of them, taken separately, to be sufficient even to discharge the current of twenty pairs, the deviations of the hydro-electric rheometer united to the induced wire remained the same, whether the two inducing wires or a single wire were employed. The arrangement which I found to succeed the best, consisted in mounting two piles of ten pairs, and making the current pass from the one into the first inductor wire, that of the other

---

* *Die Galvanische Kette Mathematisch bearbeitet,* p 36. " Des Travaux et des Opinions des Allemands sur la Pile Voltaique," *Archives de l'Electricité,* vol. i. pages 36 and 46. [Ohm's investigation here referred to will be found in Scientific Memoirs, vol. ii. p. 401.]

into the second, the direction of the two currents being the same. Thus we find, that if the two inductor currents are equal, the rheometric deviations which measure the intensities are double what they are with a single current; and if the currents are unequal, the deviations are the sum of the effects of each elementary current (*v*).

23. *Second case.*—I even employed the great helix and a pile of ten pairs. The two inductor wires were joined end to end, so that the direction of the current in the one was opposed to the direction of that in the other. The equality of their dimensions and of their conductibility involved the equality of the current which traversed each of them, a current the value of which amounted to half that of the pile. The induced currents also were perfectly equal, and as their directions were opposed, the needle of the rheometer remained quite motionless. The result was the same whether the circuit was broken or closed (*x*).

24. That the indications of the rheometer are indeed the measure of the currents simultaneously induced might be disputed. In order to reply to this objection, I substituted the metallic thermometer of Bréguet for this instrument. At the rupture as well as at the closing of the inductor currents, the needle of this very delicate apparatus remained stationary. This experiment repeated a great number of times invariably gave the same result*. I also substituted the small helix for the thermometer; small needles of tempered steel were not magnetized, in its interior, cither at the closing, or at the rupture of the inductor currents.

25. *Third case.*—This was examined by help of the great helix, of two piles of ten pairs and of different wires. I was led to the following conclusions:—

a. When the length of one of the inductor wires remains invariable, and that of the other is gradually increased, for the lengths of the additional wire increasing in geometrical progression, the intensities of the induced current measured by the rheometer diminish according to an arithmetical progression, the first term of which corresponds to the sum of the effects of the inductor wires when the length of the additional wire is null, and the last term of which is equivalent to the action of the constant inductor wire taken by itself, whether

* This experiment is interesting by its connexion with the researches of Prof. Dove on the induced currents, which, equal in rheometrical measure, produce at the same time very different physiological actions. See *Berichte der Berliner Akademie der Wissenschaften*, 1839, p. 163. *Archives de l'Electricité*, vol. ii. p. 290.

at a length of additional wire, such that the lengthened circuit is an infinitely worse conductor than the invariable circuit (λ).

b. The value of the ratio of the arithmetical progression varies with the nature and the dimensions of the additional wire employed (μ).

26. *Fourth case.*—This was studied like the second, with the apparatus mentioned (23.). The laws which govern it are—

a. For the additional lengths of the inductor wire, increasing in geometrical progression, the deviations of the rheometer, which measure the difference of intensity of the two currents simultaneously induced, increase according to an arithmetical progression, the first term of which is zero, and the last term of which is equivalent to the action of the constant inducing wire, taken by itself, even at an infinite additional length (ν).

b. From whence it results, that for additional lengths, which increase in geometrical progression, the differences between the effects of induction, produced by the two wires simultaneously, and those which the variable wire produces by itself, diminish according to an arithmetical progression (ξ).

c. The value of the ratio of these arithmetical progressions varies with the nature and the dimensions of the additional wire employed (o).

d. The presence or the absence of bars of soft iron in the helixes only modifies the intensity of the induction (π).

27. The subjoined Tables are intended to prove the enunciations (ν) and (o).

Lengths of additional wire. $q=2$.	Copper wire.			Platinum wire.		
	Deviations.		Differences. Mean error $=0°\cdot45$.	Deviations.		Differences. Mean error $=0°\cdot20$.
	Observed.	Calculated. $r=2°\cdot00$.		Observed.	Calculated. $r=2°\cdot00$.	
1	2·00	2·00	0·00	4·20	4·20	0·00
2	3·00	4·00	+1·00	6·50	6·20	−0·30
4	4·20	6·00	+1·80	8·00	8·20	+0·20
8	7·00	8·00	+1·00	11·00	10·20	−0·80
16	9·00	10·00	+1·00	13·00	12·20	−0·80
32	12·20	12·00	−0·20	14·00	14·20	+0·20
64	14·20	14·00	−0·20	—	—	—
128	16·80	16·00	−0·80	—	—	—
256	—	—	—	—	—	—

Lengths of additional wire. $q=2$.	Iron wire, No. 1.			Iron wire, No. 2.		
	Deviations.		Differences. Mean error $=0^\circ\cdot34$.	Deviations.		Differences. Mean error $=0^\circ\cdot70$.
	Observed.	Calculated. $r=1^\circ\cdot60$.		Observed.	Calculated. $r=3^\circ\cdot50$.	
1	5·50	5·50	0·00	3·00	3·00	0·00
2	7·00	7·10	+0·10	5·80	6·50	+0·70
4	9·00	8·70	—0·30	6·50	10 00	+3·50
8	12·00	10·30	—1·70	10·00	13·50	+3·50
16	13·00	11·90	—1·10	16·00	17·00	+1·00
32	14·00	13·50	—0·50	20·50	20·50	0·00
64	15·00	15·10	+0·10	25·00	24·00	—1·00
128	16·00	16·70	+0·70	29·00	27·50	—1·50
256	—	—	—	31·00	31·00	0·00

Lengths of additional wire. $q=3$.	Iron wire, No. 2.			Copper wire.		
	Deviations.		Differences. Mean error $=0^\circ\cdot40$.	Deviations.		Differences. Mean error $=0^\circ\cdot12$.
	Observed.	Calculated. $r=7^\circ\cdot00$.		Observed.	Calculated. $r=3^\circ\cdot00$.	
1	3·00	3·00	0·00	2·50	2·50	0·00
3	8·00	10·00	+2·00	4·50	5·50	+1·00
9	17·00	17·00	0·00	8·50	8·50	0·00
27	24·00	24·00	0·00	12·00	11·50	—0·50
81	31·00	31·00	0·00	—	—	—

28. The following Table will justify the assertions (ξ) and (o).

Lengths of additional wire. $q=2$.	Copper wire.			Platinum wire.			Iron wire, No. 1.		
	Deviations.		Differences. Mean error $=0^\circ\cdot50$.	Deviations.		Differences. Mean error $=0^\circ\cdot30$.	Deviations.		Differences. Mean error $=0^\circ\cdot56$.
	Observed.	Calculated. $r=2^\circ\cdot00$.		Observed.	Calculated. $r=2^\circ\cdot00$.		Observed.	Calculated. $r=1^\circ\cdot60$.	
1	16·20	16·20	0·00	12·80	12·80	0·00	11·50	11·50	0·00
2	15·20	14·20	—1·00	10·50	10·80	+0·30	10·00	9·90	—0·10
4	14·00	12·20	—1·80	9·00	8·80	—0·20	7·50	8·30	+0·80
8	11·20	10·20	—1·00	6·00	6·80	+0·80	5·00	6·70	+1·70
16	9·20	8·20	—1·00	4·00	4·80	+0·80	4·00	5·10	+1·10
32	6·00	6·20	+0·20	—	—	—	3·00	3·50	+0·50
64	4·20	4·20	0·00	—	—	—	2·00	1·90	—0·10
128	1·50	2·20	+0·70	—	—	—	—	—	—

29. These laws were also verified by substituting the magnetizing helix for the rheometer. The magnetization took place as soon as an additional length modified the equality of conductibility of the two circuits.

30. Lastly, they were verified by means of the metallic thermometer. The result of this experiment may be given as follows:—

When an induced current is the effect of two simultaneous inductor currents and of opposed directions, the one constant, the other made variable by lengths of wire which increase (exteriorly to the helix) in geometrical progression, its thermal effects decrease in arithmetical progression (ρ).

It is worthy of remark that this logarithmic law is analogous to that which M. Biot has discovered for the propagation of heat in a solid bar*.

31. *Fifth case.*—This was examined by means of the great helix, the thermo-electric rheometer, and a single pair of Daniell's battery.

The magnetized needle was never deflected, although a sort of vibration or shake, attributable to the difficulty of working in an absolutely synchronous manner the opening of one of the circuits and the closing of the other, showed the existence of induction. We conclude from this experiment that the direct induced current is equal to the inverse current (σ).

32. *Sixth case.*—Studied as the preceding, it has led to the following law:—

On lengthening the wire intended to cause direct induction by the closing of its circuit, by quantities increasing in geometrical proportion, we find that the current equal to the difference of the two opposed inductive actions increases in intensity in arithmetical progression (τ).

33. The following are the results of an experiment which establishes the preceding law:—

Lengths of additional wire. $q=2$.	Copper wire.		
	Deviations.		Differences. Mean error $=0°\cdot16$.
	Observed.	Calculated. $1=4°\cdot00$.	
1	$\overset{\circ}{3}\cdot00$	$\overset{\circ}{2}\cdot00$	$-\overset{\circ}{1}\cdot00$
2	$6\cdot00$	$6\cdot00$	$0\cdot00$
4	$10\cdot00$	$10\cdot00$	$0\cdot00$
8	$14\cdot00$	$14\cdot00$	$0\cdot00$
16	$18\cdot00$	$18\cdot00$	$0\cdot00$
32	$22\cdot00$	$22\cdot00$	$0\cdot00$

34. Many precautions must be taken in order that the experiments, the details of which I have just related, may be

* Biot, *Traité de Physique Expérimentale et Mathématique*, vol. iv. p. 669. See also Poisson, *Théorie Mathématique de la Chaleur*, p. 250 (§ 123).

considered correct. Care must be taken to prevent the additional wires from being heated by the current; without this their conductibility with small lengths ceases to be comparable to their conductibility with great lengths. The current of induction which traverses the rheometer should, as much as possible, be moderated, so that it shall not alter the relation of the magnetization of the two needles. The rheometer itself should be removed from the inductric spiral, to a sufficient distance for the electro-magnetic induction of its inductor wires not to be felt.

35. In addition, the two extremities of the wires, the immersion of which must close the simple or double inductor circuit, is to be plunged at the same instant into the mercury. It is convenient to destroy the contact at the precise instant when the needle tends, by virtue of the acquired movement, to go beyond its primitive position of equilibrium; we thus avail ourselves of the second induced current to render it immoveable, which saves much time in long experiments of this nature, and destroys the influence of the variations of intensity of the current in piles continued in action during two or three hours.

§ IV. *Influence of the state of closing or opening of the circuit induced upon the action of the inductor current on itself.*

36. There is a very great correlation between the inductor circuit and the induced circuit. When the latter is open, we know that we obtain, at the breaking of the inductor current in the mercury, very brilliant sparks, the brightness of which increases if we place a hollow pencil of iron wires in the helix, and still more if this pencil is solid. These sparks, improperly so called, are, I think, produced by the combustion and the volatilization of the globule of mercury which adheres to the extremity of the wire, and which becomes thinner in proportion as this extremity is the more removed from the surface of the liquid in its reservoir. Now, when the induced circuit is very accurately closed, these sparks diminish considerably, and even disappear altogether*. Between the two limits of open circuit and circuit perfectly closed by the aid of very short massive and good conducting bodies, there is an infinity of intermediate degrees to which sparks more or less brilliant correspond.

---

* M. Abria has also shown, that a circuit placed near the inductor does not exert any reaction when it is open. See *Ann. de Ch. et de Phys.*, vol. iii. p. 10 (September 1841). [We believe that it is perfectly well known to British observers that the sparks here alluded to are produced solely by the combustion of the mercury.—EDIT.]

37. This explains why the closure of the circuit induced by the aid of a voltaic pile diminishes the brightness of the sparks, without the direction or the intensity of the current of this apparatus having the least influence; the pile evidently acts as an imperfect conductor.

38. When the inductor circuit is closed by a voltameter in which some acidulated water for example is electrolysed, the quantity of gas obtained in the unit of time is independent of the presence or the absence of the metallic bundles [*faisceaux*] in the bobbin; it does not vary equally when we pass or arrest a continued current in any direction through the induced wire.

§ V. *Influence of the Atmospheric Pressure upon Induction.*

A. *Static Induction or Influence.*

39. Mr. Faraday, in his beautiful researches in electricity, has entered upon the question of the relation which exists between the atmospheric pressure and statical induction*. I shall relate a few of my numerous experiments upon the same subject; experiments which are only new or varied forms of those of the learned English natural philosopher, and which lead to similar conclusions.

40. I have employed a large electrical machine, the plate of which is $0^{m}\cdot85$ in diameter, and the brass conductors of which are each $0^{m}\cdot90$ long and $0^{m}\cdot108$ in diameter. Under favourable circumstances it gives sparks $0^{m}\cdot27$ to $0^{m}\cdot32$; sparks are drawn from it easily at $0^{m}\cdot11$ to $0^{m}\cdot16$ distance.

41. On operating in the dark, every time a spark is drawn from the machine, there springs a purple glow (*purpurine*) between the two balls or the two charcoal points, which terminate at an inch distance, the stems of an electrical egg void of air; but this light disappears in proportion to the return of the fluid. With my machine this luminous appearance† scarcely takes place but at a *maximum* distance of a metre from the conductors, and appears the more brilliant in proportion to the proximity of the egg. For each distance it remains the same, whether the egg rests upon an insulating support, or whether it is connected with the ground by the mediation of the hand or a metallic chain.

42. It is not even necessary to draw a spark from the con-

---

* Experimental Researches, §§ 1613 to 1616; also §§ 1359, 1405, 1526 to 1543.

† It is what Mr. Faraday calls *glow*, and what the Germans designate by *das elektrische Glimmen.* It is distinct from the spark, properly so called, *spark, brush, funke.*

ductor in order to obtain the appearance of the electric glow (*lueur*); it is observable without interruption as soon as the plate is turned rather rapidly, on placing the egg at a proper distance.

43. Upon the glass plate of the air-pump I spread a leaf of the same nature, on which I insulated a brass button of $0^{m}\cdot048$ diameter. This ball was covered with a glass funnel reversed, in the neck of which I cemented a stem, also of brass, of $0^{m}\cdot004$ thickness, and terminated below by a ball $0^{m}\cdot024$ in diameter. Between the two balls there was about $0^{m}\cdot019$ interval. The funnel was itself placed under a glass bell, bearing a leather valve and a stem which descended to $0^{m}\cdot03$ from the preceding. After exhaustion of the air, the same phænomena appeared, the plate (secondary) employed being placed in communication with the ground or insulated on a glass plate.

44. Analogous results were obtained on replacing the bell furnished with the metallic stem, *projecting exteriorly*, by an attached glass bell; the balls were then in the most perfect state of insulation and in the most perfect vacuum. I employed a double-cylinder air-pump by Babinet.

45. I have thus proved, that on placing an electrical egg near a Leyden jar of mean dimensions and charged to saturation, there is no luminous spark between the charcoal points, when the electrical explosion between the two armatures takes place instantaneously, whilst a very brilliant one is visible during the entire duration of a prolonged discharge, such as is obtained by means of a point. I believe this observation is new.

B. *Dynamical Induction.*

46. The small helix was placed on the plate of the air-pump. By means of very fine copper wires covered with silk, a communication was made between the inductor wire and piles of four or six pairs, and between the induced wire and the hydro-electrical rheometer. These conducting wires, plunged in the grease with which the broad surface of contact of the bell-glass with the plate attached was covered, did not at all prevent a very good vacuum from being formed.

47. The deviation of the needle caused by the induced current was found to be *independent* of the quantity of gas re-admitted into the receiver. The observations were repeated at different times for differences of $0^{m}\cdot025$ height of the column of mercury measuring the elastic tension, from a vacuum up to the ordinary atmospheric pressure, $0^{m}\cdot725$.

§ VI. *Examination of two circumstances under which the elec-*
tric currents and the magnets do not produce induction.

48. Since the labours of Ampère and Mr. Faraday upon
electro-magnetism and magneto-electricity, no attempts have
been made, that I am aware, to ascertain whether the presence
of a magnet or an electric current *always* induces electricity
in the neighbouring conductors, and whether induction takes
place *in the same manner in all directions*.

49. To obtain some datum upon those problems which are
at the foundation of every solid theory of induction, it was ne-
cessary to examine whether a rectilinear current induces sphe-
rical electricity around every molecule of the conductor, sup-
posed to be *electrically isotropic* (?) (that is to say, having an
equal conductibility in all directions). The experiments of
Mr. Faraday had shown us that induction takes place in a
wire placed *parallel* to the inductor wire, whether the two
wires were or were not twisted into a helix. My experiments
have proved to me, that induction does not take place in a
sensible manner in a wire which is at a right angle to the cur-
rent.

50. I arrived at this conclusion by the two following me-
thods:—I placed the brass wire No. 1 in the small hollow
helix for magnetizing, and after having united the extremities
of the latter with the thermo-electric rheometer, I closed with
a wire the circuit of a strong voltaic element. At the break-
ing, as at the closing of the circuit, the needle remained per-
fectly immoveable, when even a bright spark upon the mer-
cury proved the intensity of the current and its induction upon
itself.

51. I substituted for the preceding apparatus an electro-
magnet, and I closed the circuit of the pile by the iron of the
instrument. The needle of the rheometer, which communi-
cated with its external helix by copper wire and by insulated
spirals, one with another, in no degree deviated, even on
placing in the circuit a large spiral plate (*coil*).

52. Thus a voltaic conductor induces currents in neigh-
bouring conductors only parallel to its own direction. It is
from the parallelism between the currents which surround the
magnets (according to M. Ampère) and the coils of the ex-
terior helix of the electro-magnet, that the approach of a mag-
netic body near soft iron determines induced currents in the
helix, and that the passage of the current in the latter mag-
netizes the bar. The same fact also accounts for the action
of the voltaic current on the magnetized needle,—an action
discovered by M. Oersted, and which is the foundation of
electro-magnetism.

53. It remained to examine whether the presence of a mag-
net or of a conductor traversed by a current always induces
electricity in a neighbouring conductor, placed in a convenient
manner ? To this question the reply must be *negative*.

54. We know that if a natural or artificial magnet, or an
electro-dynamic helix, is brought near to an electro-magnet
of soft iron, the latter, on taking the magnetic power, produces
an instantaneous current of induction in its helix. But when
the approach is made slowly, the developed current loses its
intensity, and by increasing or diminishing the distance of the
soft iron from the magnet by very slow degrees, we succeed
in completely destroying all sensible induction.

55. The first experiments were made with a horse-shoe
iron magnet, capable of supporting eight kilogrammes; it was
suspended by a metallic chain connected with the arbor of a
lathe. An electro-magnet was fixed solidly on the table under
the magnet, and its helix fastened to the thermo-electric rheo-
meter. On turning the winch very slowly, I succeeded in
bringing the magnet to within $0^m \cdot 004$ distance of the electro-
magnet without any induction in the helix; but from that
point the attraction of the magnets having brought them in
contact from the extensibility of the chain, the needle was vio-
lently deflected.

56. I then used a magnet formed of seven horse-shoe iron
plates, capable of raising nearly forty kilogrammes. The
electro-magnet was no other than one of the keepers of the
magneto-electric machine, of which the magnet formed part.
On employing the same rheometer, I succeeded in bringing
to the point of contact, and of separating to a distance of se-
veral millimetres, the two pieces *without any sensible induction
resulting*. I operated by means of a screw which moved very
slowly the slide upon which the magnet was solidly retained.
The needle of the rheometer deviated several turns as soon as
I rapidly caused a variation, although of a small arc, in the
position of the keeper parallel to the polar surfaces of the
magnet, at any of the distances at which the experiment had
been made.

57. I arrived at the same result on employing the voltaic
current as the cause of induction. I placed a cylindrical
copper ring in some water containing $\frac{1}{10}$th of sulphuric acid,
so that its geometrical axis was vertical. This ring was con-
nected with one of the extremities of the wire of the rheometer,
and to the other extremity I soldered a very thin plate of
amalgamated zinc, cut into the form of a very slender tongue.
This plate, being immersed in the direction of the axis of the
ring, produced an induced current very nearly null when the

immersion was made very slowly, whilst it occasioned one measured by 40° of sudden deviation when the immersion took place rapidly.

XLV. *Addition to a former communication on the Diffusion of Gases.* By THOMAS S. THOMSON, *Esq.*
To Richard Taylor, Esq.

DEAR SIR,

IN following up the ideas contained in my paper on the Diffusion of Gases, which you had the kindness to lay before the public (pres. vol. p. 51), a subsequent consideration has occurred to me which I now send you for publication. The chemical constitution of the atmosphere is twenty-one parts by measure of oxygen and seventy-nine parts by measure of nitrogen. Now supposing a tube twenty-one miles in length and another seventy-nine miles, separated from each other by a perfectly moveable diaphragm, the former filled with oxygen and the latter with nitrogen gas, sound would travel along this tube in a certain appreciable space of time which can be calculated as follows. The data I extract from Poisson's *Mécanique*.

$$a = \text{velocity of sound}$$
$$g = 9^{\text{m}}\cdot80896$$
$$h = 0^{\text{m}}\cdot76$$
$$\frac{m}{\Delta} = 10\cdot462$$
$$\alpha = 0\cdot00375$$
$$\theta = 15°\cdot9 \text{ (centigrade)}$$
$$\gamma = 1\cdot3748.$$

The velocity of sound is expressed by the following formula:

$$a = \sqrt{\frac{g\,m\,h\,\gamma}{\Delta}(1 + \alpha\,\theta)}.$$

I mean now to assert that if, by the removal of the diaphragm, the gases be permitted to diffuse themselves through each other, sound would still travel in precisely the same time from one end of the tube to the other.

It is a necessary consequence of D'Alembert's principle, which I extract from Poisson,

$$A\,m\,P + A'\,m'\,P' + A''\,m''\,P'' + \&c. = 0.$$

The direction which molecular chemistry is taking at the present day renders it necessary to recall to the minds of some of your readers, that the repulsion of gaseous particles does not obey the law of the inverse of the square of the distance, but the inverse of the distance itself. It forms the subject of

one of Sir Isaac Newton's propositions in the Principia, but seems entirely to have escaped the notice of Laplace, Poisson and the other French mathematicians.

Your insertion of this in your ensuing Number of the Philosophical Magazine, will oblige

Your obedient Servant,

Primrose, September 5, 1844. THOS. S. THOMSON.

XLVI. *Method of preparing Benzoic Acid.* *By* Dr. STENHOUSE *.

PROCEED as in Scheele's process, by mixing finely pulverized gum-benzoin very intimately with its own weight of slacked lime, also in fine powder. The mixture is to be repeatedly boiled with water so long as any benzoate of lime is dissolved. The filtered liquid is to be evaporated to about one-sixth of its bulk, and a considerable quantity of a strong solution of hypochlorite of lime is to be added to it. The mixture, while boiling hot, is to be decomposed by a slight excess of muriatic acid, and the boiling continued till all the chlorine is driven off. On the cooling of the liquid the benzoic acid is deposited in crystals, which are but very slightly coloured, the greater portion of the colouring matter having been removed by the chlorine. The crystals, however, still retain some resinous matter, from which impurity they may be readily freed by being once or twice dissolved in a very small quantity of boiling water and recrystallized. A little purified animal charcoal may also be advantageously employed for this purpose.

Glasgow, September 19, 1844.

XLVII. *On the successful repetition of an Experiment in which Electrical Tension is produced without Metallic Contact. By* J. P. GASSIOT, *Esq., F.R.S.*

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

VOLTA'S original experiment of the separation of two discs of dissimilar metals insulated by glass handles is well known to the electrician : the cause of the electrical phænomena which are thereby produced, has been the subject of much discussion. The author of the article 'Galvanism' in the Ency. Metrop. p. 210, alludes to it as follows:—" It does not accord with any of the previously known laws of electrical equilibrium, and appears to be totally at variance with every

* Communicated by the Author.

preconceived notion we might have entertained of the results of any disturbance in that equilibrium; we must, therefore, for the present content ourselves with admitting that some un-known cause exists, which upon the contact of zinc and cop-per determines the transfer of the positive and negative elec-tricities respectively to the two surfaces which are brought in apposition."

Early in 1843 Professor Grove devised an experiment, a de-scription of which appears in the reports of a lecture given by that gentleman at the London Institution, on the 18th of January of that year: the experiment is thus described in the Literary Gazette of the 21st of the same month:—" Discs of zinc and copper were juxtaposed surface to surface, contact being pre-vented by a rim of paper, and yet when separated the electro-scope was affected: here then was *no contact*, and yet the elec-trical effects were produced, which should be so by the mutual radiation, but which could not be by the contact theory."

Mr. Grove thinks the electrical phænomena due to a radia-tion similar to that which takes place in the experiments of Moser and others, and which produces a change, either phy-sical or chemical, upon the surfaces of the plates.

On the 10th of February in the same year Mr. Grove re-peated the experiment at the Royal Institution (Electrical Magazine, vol. i. p. 57), but still by many it has not been considered an unexceptionable experiment; and, considering its important bearing on the contact theory, the objections that have been raised are certainly deserving of attention: by some, it has been inferred that the mere interposition of a *rim* of paper might not have prevented actual metallic contact in the portions of the discs which were not similarly protected; by others, it is somewhat fairly assumed that a certain amount of friction must be produced by the pressure of the discs against the paper, and that this would in itself be sufficient to account for the very slight signs of tension which were deve-loped, although this objection would equally apply to the ex-periment of Volta.

It appears to me rather extraordinary, considering the im-portance of the experiment, that no further attempts have been made to render it unexceptionable.

In the Phil. Trans. for this year [*] I have described an elec-troscope by means of which the tension in a single pair of the voltaic battery is rendered distinctly appreciable.   Since its publication it has occurred to me, that by means of this in-strument I could repeat Mr. Grove's experiment without the objectionable points to which I have alluded, and after a few

[*] See the following paper.—Edit.

trials I found the following arrangement produce the effects I had anticipated.

Two plates, one of copper and the other of zinc, 4 inches in diameter, were attached to the insulated glass pillars of my micrometer electrometer (Phil. Trans. 1840, p. 185); the plates were carefully approximated to about $\frac{1}{100}$th of an inch apart: when thus adjusted, a copper wire was attached to each of the plates, and also to the discs of the electroscope, which are fixed at about one-eighth of an inch apart; the leaf of the electroscope was raised so as to allow it to swing clear of the two discs, and when not excited, to remain equidistant from each : thus arranged, the apparatus is ready for the experiment. With one hand the experimentalist holds a Zamboni's pile so as to have one of its terminals within about an inch of the brass plate or cap of the electroscope, and with his other hand he separates the plates; immediately on separation the terminal of the pile is brought into contact with the cap of the electroscope, and the leaf will be attracted as follows:—if touched by the — terminal of the pile, the leaf of the electroscope will be attracted to the disc in connexion with the zinc plate, and if by the + terminal, the leaf will be attracted to that in connexion with the copper plate, which are precisely the same results as follow the separation after actual contact.

In this experiment we certainly have decided signs of electrical tension without any metallic contact.

Clapham Common,   I am, &c.,
September 24, 1844.   JOHN P. GASSIOT.

XLVIII. *A description of an extensive series of the Water Battery; with an Account of some Experiments made in order to test the relation of the Electrical and the Chemical Actions which take place before and after completion of the Voltaic Circuit.* By JOHN P. GASSIOT, *Esq.,* F.R.S.*[*]

1. IN a paper, which I communicated to the Royal Society in 1839, and which was honoured by insertion in the Transactions of the following year[†], I described a series of experiments made with some powerful voltaic batteries, for the purpose of determining the possibility of obtaining a spark *before* the completion of the voltaic circuit. I was therein enabled to establish a few facts respecting polar tension, or rather

[† Noticed in Phil. Mag. S. 3. vol. xvi. p. 594.]

respecting the absence of any notable degree of it in the batteries I described*; for instance, I proved that, with 320 series of Prof. Daniell's constant battery, polar tension was not evinced adequate to the striking distance of $\frac{1}{3000}$th of an inch; nor was I more successful in obtaining it with a water battery of 1024 series†, constructed by the same gentleman. I also stated that, according to the present theoretical views of the action of the voltaic battery, with the apparatus I then used, it ought to have taken place; and that, if by a still more powerful apparatus it could not be obtained, the theory must, in some way or other, be incorrect.

2. The preceding negative facts are not without their value in a scientific point of view; they show us, at least, a certain limit within which the anticipated effects could not be obtained. At the same time I could not fail to admit that they were anything but conclusive, as to the actual question of the possibility of obtaining the spark before the circuit was completed. That I am justified in calling the spark, under such circumstances, an anticipated effect, may be fairly assumed, because every electrician is aware that the terminals of a voltaic series invariably evince a certain amount of tension‡; and as spark is but a consequence of tension exalted to a maximum, it is only fair to anticipate that, by increasing the tension, it would be obtained.

3. A short time after the publication of the paper to which I have alluded, a communication reached me from my friend Mr. Crosse, of Broomfield, Somersetshire, wherein he stated that, with 1626 cells of copper and zinc, excited with river water, he had succeeded in obtaining a spark between slips of tinfoil pasted on sealing-wax. This communication I immediately forwarded for publication in the Philosophical Magazine for September 1840.

4. I was at that time engaged in the construction of an extensive series of the water battery; but, from the difficulties of insulation, which continually presented themselves, considerable time elapsed before the battery was in a condition to afford results, on which any dependence could be placed. Having, at length, to a certain extent, surmounted these difficulties, I have been enabled especially to study the character and action of the water battery, and through it, I may hope, of the voltaic battery generally. The results of my experiments, with a description of the apparatus used, may, I think, not be uninteresting to the electrician, particularly as they

---

* Philosophical Transactions, 1840, p. 184, § 10.
† Ibid. § 15.   ‡ Ibid. § 14.

establish the fact, beyond any doubt, of not only the passing of a distinct spark before the completion of the circuit, but the practicability of continuing this action for several weeks in constant succession, and enabling us to examine, with much accuracy, the rationale of the action of this extraordinary apparatus.

5. The water battery, which I have constructed, and which I am about to describe, consists of 3520 pairs, or series of copper and zinc cylinders, each pair being placed in a separate glass vessel, well covered with a coating of lac varnish. The glass cells are placed on slips of glass, covered on both sides with a thick coating of lac; this coating being fixed by heating the glass over a gas furnace, and then covering it with the varnish. The 3520 cells, thus insulated, are placed on forty-four separate oaken boards, also covered with lac varnish, each board carrying eighty cells. The boards, or trays, slide into a wooden frame, where they are further insulated by resting on pieces of thick plate glass, similarly varnished.

6. It may at first sight appear that many of these precautions are unnecessary; in truth, had I, at the outset, expected they would have been requisite, I might probably have been deterred from attempting so troublesome and lengthened an inquiry. At first I imitated the apparatus of Mr. Crosse, already referred to (3.), the copper cylinders being made watertight that they might themselves constitute the cells; and considering such insulation would be sufficient, I attached the cells to the boards with sealing-wax, poured into holes made in the boards for the insertion of each cell. I found this arrangement answer very well for a few hundred series, but when the number was augmented, and the battery completed, the insulation was sadly deficient. This induced me to take the battery asunder, and to have distinct or separate glass vessels made for each pair. After again completing the entire series, I found the insulation was even less efficient than before; for from the glass attracting moisture from the atmosphere, as well as from the evaporation of the battery, each cell became a conductor, and scarcely any effect could be produced on the *electroscope*; in fact, it was not until I had finally adopted the arrangements I have previously described, that any approximation to a tolerable insulation could be maintained.

7. The general appearance of the battery may be seen by referring to fig. 1, where A, A' represent the wooden frames, in which are placed the forty-four boards containing the entire battery; B a shelf on which a galvanometer can be placed, or any other apparatus for the detection of a current or chemical action. N and P are the terminals or poles of the battery.

Fig. 2 is a single cell; g, a glass vessel; c, copper; z, zinc. Fig. 3, one of the boards as it appears when removed from Fig. 1.
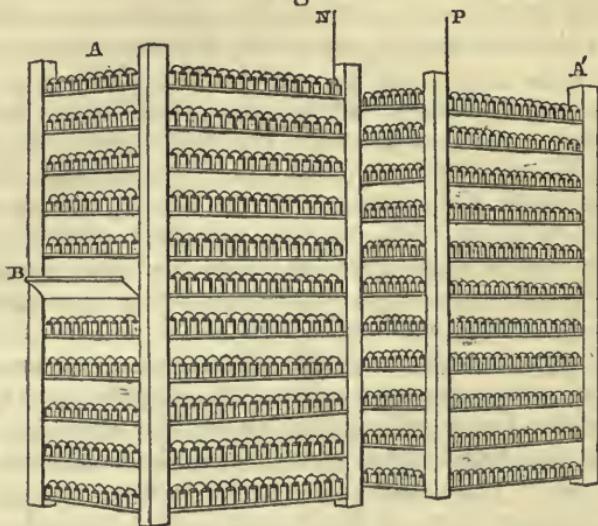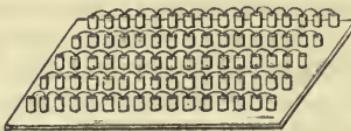
Fig. 2.   Fig, 3.   Fig. 6.
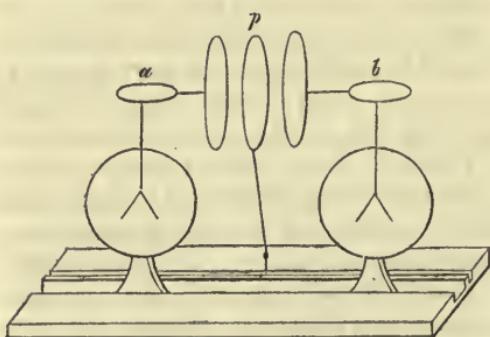
Fig. 4.   Fig. 5.
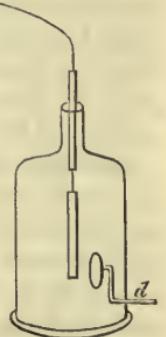
the entire series. Fig. 4, a double electroscope. Fig. 5, a Harris's single-leaf electroscope. Fig. 6, another electroscope, having two separately insulated gold leaves, $b\,b'$: in addition, I used a delicate galvanometer, and a solution of iodide of potassium for the detection of currents and of chemical action. The battery was charged, by carefully filling each cell with rain water.

8. With all the precautions I have described, the insulation of the battery was still imperfect; and, from the experience which I have gained during the construction of this apparatus, I have little hesitation in asserting, that the very nature of the water battery must prevent the experimentalist from obtaining insulation for any lengthened period, when such an extended series is employed.

9. In proceeding to describe the effects which this apparatus has presented, I must endeavour to draw a distinct line of demarcation between the *static* and the *dynamic* effects; for although these are, in a certain sense, both associated in some forms of electric development, yet as I have been enabled in a degree to isolate them here, it is my intention to regard them separately.

10. We know, from the very earliest experiments, that the extremities of a voltaic pile present opposite electrical states; it is therefore stating no new fact, to say that when one extremity of the series is connected with the ground, the other, on being connected with a gold-leaf electroscope, indicates a high degree of electric tension, and that the gold leaves diverge with considerable energy. Indeed, in the battery above described, there was little need of making connection with the ground; for, with all my precautions, I found the insulation was in a short time very imperfect, and that, by this communication with the earth, a complete circuit, to a certain extent, already existed.

11. As the *static* effects present themselves antecedently to the *dynamic*, they necessarily demand the first notice. The entire battery was connected in one series, and copper wires from the extreme cells were connected with the plates a and b of the double electroscope (fig. 4); this instantly produced a considerable and steady divergence of the gold leaves; and, on applying the usual tests, the plate b, connected with the copper extremity, gave signs of vitreous, and a, connected with the zinc, of resinous electricity. If a was connected with one extremity of the battery, and the other extremity was connected or not with the ground, the same general effects occurred; the divergence of the leaves corresponded with the connection, and the leaves of b diverged by induction; if, in this state, b was touched and then removed from the influence of a, it was found charged with the opposite electricity.

12. These inductive effects were obtained under other forms; for instance, the condensing plate (p), which had been removed during the preceding experiments, was opposed to the charged plate a. When a alone was connected with the battery, and p was touched, while under the influence of induc-

tion, and then removed, p was found oppositely charged; as was also an insulated carrier ball, when similarly treated.

13. The plate a of the electroscope (fig. 4) being retained in connection with one end of the battery, a piece of very thin mica was laid on it; on this mica rested a wire W proceeding from a single-leaf electroscope (fig. 5), the disc (d) of which was in connection with the earth by means of a wire; with this arrangement the gold leaf was electrified by induction, and struck against the disc (d); glass, lac and sulphur were in turn substituted for the mica, and the same general effects resulted. The same battery connection was maintained, the electroscope (fig. 5) being disconnected. An insulated carrier ball was successively applied to the plate a, and an unlimited succession of charges could be carried and accumulated to another electroscope: these charges were with equal facility obtained when the *direct* connection between the battery and the earth was broken; the insulation of the battery was, however, comparatively imperfect; and I shall therefore have occasion to revert to some of these results which I afterwards obtained less in degree, but equally definite in character, from a portion of the battery detached for the purpose of more efficient insulation; for the present we need only allude to those effects, which do not absolutely involve the perfect insulations of the battery itself. When a Leyden jar, held in the hand, was subjected to the action of one end of the battery, a charge was readily accumulated, and, of course, still more favourably by means of a mica battery. When the coatings of a Leyden battery, consisting of twelve jars, with a surface of sixteen feet, were connected with the respective ends of the series, the accumulation of tension was considerable.

14. With the entire battery, the tension was so great, that the leaves of a gold-leaf electroscope diverged when that instrument was placed within two or three inches of either end of the battery, or over any of the terminal cells. Advantage was taken of this to test, whether any effect of tension could be observed when the circuit was completed; but the instant this was effected, the leaves of the electroscope as instantly collapsed, nor could I detect, either by the aid of the condenser, or otherwise, the slightest trace of tension; it, however, immediately reappeared when the circuit was again broken.

15. Thus far I have been examining the *static* effects of a moderate amount of tension, similar in kind to those which have been long familiar to the electrician, but modified so as to produce inductive effects, differing in some degree from any elsewhere recorded. My first experiment, after making every

allowance for loss of electricity, or, as it would be better to express it, loss of tension through insufficient insulation, admits but of one interpretation : the interpretation itself is generally allowed, but the force of it is not, I believe, generally admitted ; it is, *that the elements constituting the voltaic battery, when arranged in series, assume polar tension before the circuit is completed*; and that in the apparatus above mentioned, this tension is such *that a spark will pass before the circuit is completed*.

16. When the micrometer electrometer, described in my former paper*, was introduced between the terminal wires, sparks, through the space of $\frac{1}{30}$th of an inch, were obtained ; and when the double electroscope (fig. 4) was included in the circuit, and the discs *a* and *b* were approximated, an uninterrupted succession of sparks would pass between the discs. These effects, which I have repeatedly shown to many friends, are most brilliant. On one occasion they were continued uninterruptedly day and night for upwards of five weeks ; and although some months have now elapsed since this battery was completed, it still exhibits the same effects.

17. When the experimenter was standing on the ground, and consequently, as has been already explained, in actual, although imperfect connection, with the battery, he could draw sparks from either terminal. We shall, in the sequel, be enabled to trace, with more precision, the rise of this tension ; for the present, we are only concerned in establishing its existence, and thereby proving the first fact, that *tension* or *electro-static effects precede, and are independent of, the completion of the voltaic circuit*.

18. Hitherto we have not obtained any insight into the condition of the *dynamic* effects under such circumstances of antecedent tension. For testing the presence of what is usually termed the current, or in other words, obtaining the means of observing the electro-dynamic effects, I used the instrument which is best suited for examining such phænomena, and which invariably attests the instant completion of a voltaic circuit. An exceedingly delicate galvanometer was introduced at B (see fig. 1), and the two condensing plates *a* and *b* of the double electroscope (fig. 4) were respectively attached by wires to the terminals N and P of the battery, fig. 1. If great care was taken not to make any connection with the ground, the party manipulating being himself well insulated on shell-lac, no action could be perceived on the needle in the galvanometer, although the gold leaves of the electroscope immediately diverged to a very considerable extent.

* Philosophical Transactions, 1840, p. 184, § 12.

19. This experiment was, however, of too much importance to be passed over without adopting every means of making it unexceptionable. Two trays from the battery, being a series of 160 cells, were removed and insulated, by being supported on stout varnished glass pillars twelve inches high; the whole being placed upon an Arnott's stove in which a fire had been kept burning for several hours. The galvanometer was interposed between the zinc terminal of one tray and the copper terminal of the other; and the extremities of this reduced series were arranged so as to exhibit the same effects of electric tension which we have seen in the entire series; but not the slightest indication of dynamic action could be detected by the galvanometer. The action of the instrument I used could not be in fault; and some idea may be conceived of its extreme delicacy, when I state that, with one cell of the gas battery* I have obtained a steady deflection, whilst a resistance of twelve miles of thin copper wire was interposed in the circuit; when the electroscope (fig. 6) was used, $c\,c'$ being respectively connected with N and P of battery, the gold leaves $b\,b'$ were attracted; and the moment they touched each other, the needle of the galvanometer was deflected.

20. The best definition that occurs to me of a current is that given by Faraday in his Third Series of Experimental Researches†. "By current I mean anything progressive, whether it be a fluid of electricity, or two fluids moving in opposite directions, or merely vibrations; or, speaking still more generally, progressive forces;" and in juxtaposition to this, he says‡, "If the magnetic effects depend upon a current, then it is evident they could not be produced in any degree before the circuit was complete, because prior to that no current could exist." Now it is manifest that, in the experiments already mentioned, the voltaic elements have the power of exhibiting electric effects at either, and both ends or terminals, before any progression or actual perceptible force takes place in the course of the series; in other words, that *static* effects exist before or independently of currents, but that these effects cease immediately on currents being developed.

21. But, in an inquiry like this, we must examine more closely the actual chemical action; for it involves much of the source of controversy between the contact and chemical theories; and I was naturally anxious to discover whether simultaneously with these *static* effects, or perhaps antecedently to them, *any chemical action took place in the cells of the battery*; and if so, to what amount.

---

* Philosophical Transactions, 1843.
† Experimental Researches, § 283.　　　　‡ Ib. § 282.

22. Faraday[*], in the course of that branch of his experimental researches which introduced into notice the voltameter, established the most accurate means of measuring the amount of chemical action in the battery cells, by the equivalent amount of chemical decomposition exhibited in that instrument; he has also directed our attention to the fact, that this amount of electro-chemical decomposition depends essentially on the current as denoted by the galvanometer. There was every reason, with these facts before us, to expect that the battery, which did not produce a current, would not evince any degree of chemical decomposition. Recourse was, however, had to the test of experiment; and, instead of introducing the galvanometer, I substituted for it a small piece of bibulous paper, saturated with a solution of iodide of potassium; the gold leaves of the electroscope diverged as before, but, however long the duration of action, not the least indication of the liberation of iodine was perceptible. The inference I make from this is, that no definite chemical action took place in any cell of the battery, and consequently that the *electric effects* above shown, and which are termed *static* effects, take place *before* or *independently of the actual development of the chemical effects*.

23. I am aware that, in some form or other, this fact has been acknowledged by the most strenuous advocates of the chemical theory. Becquerel[†] thus adverts to it:—" We must conclude, from all the electrical phænomena, that, in almost all cases, a chemical action has taken place; and hence we are led to believe that the latter is the cause which exercises the greatest degree of influence over their production; nevertheless, in the present condition of science, we must not yet abandon Volta's theory, since it may very readily happen, that, at the contact of two bodies, a disengagement of electricity may take place, resulting from a commencement of chemical action between these bodies."

De la Rive[‡], who has laboured perhaps more than any of the continental philosophers in advocating the chemical origin of voltaic electricity, cannot but admit some such similar qualification, as may be seen from the following extract from one of his papers:—" The two theories between which philosophers are divided with respect to the origin of voltaic electricity, are still the subject of lively controversy; when we are adverting to the pile itself and to hydro-electric currents, we cannot deny the superiority of the chemical theory; the recent

* Experimental Researches, § 510.
† *Traité de l'Elect.*, vol. ii. p. 145; vol. v. p. 3.
‡ *Archives de l'Electricité*, vol. i. p. 619.

labours of Faraday have, moreover, added powerful arguments in favour of this theory; but it must be acknowledged that it is not easy to defend it when we advert to electricity of tension developed in the contact of two heterogeneous bodies, especially if the two bodies are solid."

Faraday*, who has recently instituted a series of elaborate researches in support of the chemical theory, writes thus of it:—"The theory assumes that the particles of the di-electric (now an electrolyte) are, in the first instance, brought by ordinary induction into a polarized state, and raised to a certain degree of tension or intensity before discharge commences, the inductive state being in fact a *necessary preliminary* to discharge." Again, "One point is, that different electrolytes or di-electrics require different initial intensities for their decomposition. This may depend upon the degree of polarization which the particles require before electrolytic discharge commences†."

Grove‡, in a recent communication to the Royal Society, in allusion to the action of the gas battery, says, "If, indeed, the contact theory assume contact as the efficient cause of voltaic action, but admit that this can only be circulated by chemical action, I see little difference, save in the mere hypothetical expression, between the contact and chemical theories; any conclusion which would flow from the one would likewise be deducible from the other; there is no observed sequence of time in the phænomena, the contact or completion of the circuit and the electrolytical action are synchronous. If this be the view of contact theorists, the rival theories are mere disputes about terms. If, however, the contact theory connects with the term *contact* an idea of force which does or may produce a voltaic current independently of chemical action, a force without consumption, I cannot but regard it as inconsistent with the whole tenor of voltaic facts and general experience."

24. I shall have occasion to revert to the gas battery, the action of which is fully described in the paper from which I have taken the preceding quotation; but the action we are now examining is not that arising from *contact or completion of the circuit*, but that which is caused by contact in the arrangement of a progressive series of the elements of a voltaic battery *previous* to the circuit being completed; such progressive arrangement being indispensable for the production of the effects we are now examining, or the production of certain forces in a given direction. A mere heterogeneous assemblage

* Experimental Researches, § 1345. † Ibid. § 1354.
‡ Philosophical Transactions, 1843 [and Phil. Mag. S. 3. vol. xxiv. p. 427.].

of the elements will not produce the effects; on the contrary, any alteration in the regular series produces a corresponding reduction of force, and any number of batteries arranged— copper, liquid, zinc,—will exactly neutralize the action of a similar number arranged,—zinc, liquid, copper.

25. The question I am now examining is, however, not whether contact produces current, but whether it produces electric development, and whether that development is accompanied by any chemical action; and this necessarily involves the question, whether the effects of current, or those usually called voltaic, have the same efficient cause as the electrostatic. That certain effects can be produced independently of any *apparent* chemical action, is not merely proved by experiment with the water battery, but it is further confirmed by many unexceptionable experiments; among those for which I may particularly claim attention, are those of M. Peclet, who in describing them says, "I have only had in view static electricity developed by contact, I have not troubled myself about currents when the circuit is closed*."

26. The examination of M. Peclet's experiments, the investigation of the action of the voltaic series generally, and more particularly of the water battery, seem to lead to the inference that elective affinity is greatly concerned in the *antecedent* action, of which chemical combinations, when the circuit is closed, are the *consequence*. I might quote the particular experiments of M. Peclet as an evidence of static effects having been obtained without any apparent chemical combination; but, without attempting to follow out what may, perhaps, somewhat fairly, be assumed as hypothetical cases, viz. those in which tension does not appear to terminate in chemical action, *let us trace the process by which tension rises in an insulated pile, and the still further process necessary to establish a current.*

27. Two trays of the battery were carefully insulated as before (19.): if in this state one of the piles is touched by the hand, its electric tension is apparently destroyed, the leaves of the electroscope in connection collapsing; whilst those of an electroscope attached to the other terminal obtain their extreme divergence. If this battery be now left to itself, the end which has been touched regains a certain amount of tension, and the leaves of the other electroscope collapse in proportion: generally, the means of raising the tension of one extremity of the battery is to touch the other. I do not mean to assert that the tension of the end which was touched is entirely destroyed, but certainly with 160 series, for the space of

* *Archives de l'Electricité*, vol. i. p. 622.

several seconds, I could not with the most delicate gold-leaf electroscope obtain the slightest indication of it.

28. Marianini went through a series of experiments, in which he found that in no case was tension actually destroyed; but that it fell in proportion to the duration of the time during which the circuit had been completed. My object now was not, however, to complete the circuit; for I wished to test an intermediate stage between actual insulation and actual completion of circuit, in order to discover the character of the discharges effected by touching the respective terminals, or rather their influence over the state of tension, into which the whole series was thrown. I wished, for instance, to discover whether the discharge I effected, threw the elements of the battery *back*, into the normal condition which prevailed antecedent to the discharge; or whether it threw them *forward* by completing the act of chemical combination, the preparatory state of which already existed; whether, for instance, mere discharge, either by the earth or by completion of the circuit, for an instant, is one small fraction of a current; and, if so, whether a current is not really a collection of discharges of electricity of tension.

29. For this purpose the following experiments were made: a copper wire attached to N of the battery was connected with the galvanometer, and this with the plate a of the double electroscope, fig. 4. A platinum wire was attached to P of the battery, the end of which rested on a piece of bibulous paper saturated with a solution of iodide of potassium: another platinum wire, also resting on the paper, was connected with the

Fig. 7.
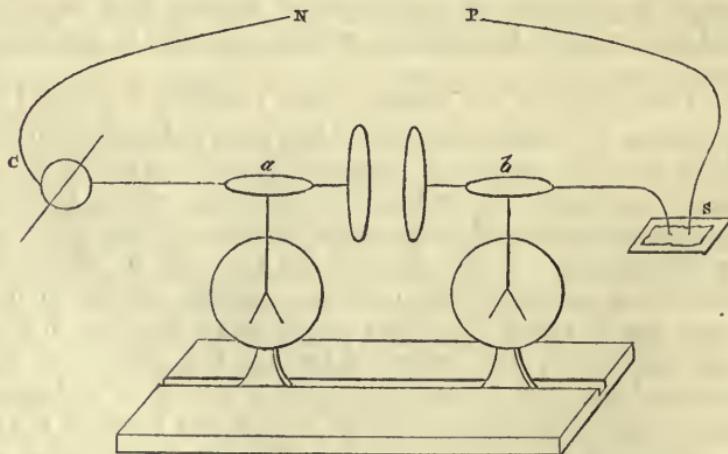
other plate b of the electroscope; by the mechanical arrange-

ment of this instrument, the plates could be approximated or separated as required. Fig. 7 shows the arrangement; N and P, the terminals of the battery; G, the galvanometer; *a* and *b*, the plates of the electroscope; S, solution of iodide of potassium.

30. When the plates *a* and *b* were approximated so as to permit sparks to pass at intervals of about a second, a *tremulous* motion was imparted to the needle; but when they were brought so nearly in contact as to permit the discharges to take place in quick succession, the needle was *steadily* deflected, and iodine freely evolved; proving that chemical action was occurring in each cell, and that the current is a collection or an accumulation of discharges of electricity of tension; for although the circuit was completed by distinct and separate discharges, still the deflection of the needle was as steady as if the discs had been in actual metallic contact.

31. The discs *a* and *b* were then separated to the distance of about an inch; a piece of tinfoil was suspended by means of a thread of white silk; the tinfoil vibrated between the plates, but no effect could be produced on iodide of potassium or on the galvanometer.

32. I have already alluded to the extreme difficulty I experienced in effecting perfect insulation. I was anxious to ascertain whether this insulation, and at the same time a high state of intensity, could be obtained, and whether, in such a state, any evidence of chemical action could be detected when one end of the battery was in connection with the ground. After many trials, I succeeded in insulating 320 cells; and so efficient was even this reduced number in point of intensity, that sparks could be obtained by means of the micrometer electrometer through a space of $\frac{1}{1000}$th of an inch. The ends of two platinum wires were then fixed so as to rest on a piece of bibulous paper saturated with a solution of iodide of potassium. One of the wires was connected at one time with the copper, and at another time with the zinc terminal of the battery; while the wire attached to the other extremity was connected with the earth. This arrangement being continued for several hours, and the paper kept constantly moist, not the slightest evidence of any evolution of iodine could be detected, until by a momentary contact made with the further extremity of the battery and the ground, either by a wire, or by touching it with the hand, the circuit was completed. Here we have a voltaic battery the intensity of which is sufficient to elicit a *spark* between its terminals before the circuit is completed, but in which antecedent thereto, the slightest chemical action is not appreciable; and this when one end is in con-

nection with the earth, and consequently having the intensity exalted to a maximum short of actual discharge.

33. I am aware of the many forms the contact theory has assumed since Volta first propounded it; nor can we lose sight of the many errors which his successors have committed in attempting to follow out their favourite views of metallic contact and electro-motive forces. I am indebted for much information on this point to the kindness and industry of Mr. C. V. Walker, who has occupied much time in procuring me references to the researches of those electricians already alluded to, as also to a long list of others whose names adorn the pages of science. I could easily have added to those I have named, by referring to the experiments of many other authors who consider that their favourite theory of contact has been fully established. It is not, however, my object to advert to these speculations: we have before us certain facts connected with the action of the voltaic battery; first *contact*, or successive juxtaposition, and simultaneously with it, effects of *tension*; then the completion of the circuit, and simultaneously with it, *development of chemical action* and *current*; but in all these cases contact is first in order, developing tension. Becquerel describes it by stating, " When two bodies are in contact the affinities commence exercising their action before combination takes place." M. De la Rive, "The electricity of tension developed by contact of heterogeneous bodies." Faraday, " That the particles of the electrolyte are brought into a polarized state, and raised to a certain degree of tension, before discharge takes place:" but I am not aware that there are any *experimental* facts to prove whether this tension in the voltaic battery, which itself is of an opposite character at each terminal, and, as we have seen, can be exalted so as to produce discharges in the form of sparks for many weeks' constant duration (16.), is due to the chemical constituents of the battery, or to mere contact of dissimilar bodies, without reference to their chemical affinities. I was therefore most anxious to see whether some experiment could not be devised, which would test the action in a satisfactory manner, and for this purpose I availed myself of an extended series of Grove's gas battery, described by him in a paper already alluded to (23.)*.

34. The elements used in this form of the voltaic battery consist of two gases and one metal; with fifty series charged with oxygen and hydrogen, it is stated that the gold-leaf electroscope was notably affected. The instrument used by Mr. Grove, whose original experiments I had the pleasure of witnessing in the laboratory of the London Institution, was

* Philosophical Transactions, 1843.

the same as is represented in fig. 6. Nothing can be more decisive than that the *static* effects of the voltaic battery are not due to the contact of dissimilar *metals*; as in this arrangement, only one metal, platinum is used; but this battery, whose action is, in itself, so purely chemical, presents to us a full corroboration of the *static effects preceding the development of chemical action*.

35. A series of forty cells, the same as are described in Mr. Grove's paper *, were charged with oxygen and hydrogen; these sensibly affected the gold-leaf electroscope, fig. 3. This arrangement was kept charged for upwards of three months. No decrease in the gas of the oxygen tubes could be detected. Whenever the terminals were tested by the electroscope, they invariably exhibited the usual signs of tension; but not the slightest chemical or dynamic effect could be obtained until the entire circuit was completed.

36. Here we have a battery, the active elements of which are two gases, which, with a closed circuit, immediately enter into active chemical combination; remaining for upwards of three months in such a state of tension as at all times to affect the leaves of an electroscope; but in which no amount of chemical action could be detected whilst the circuit remained open.

37. It now became a matter of some interest to ascertain, *first*, the minimum amount of series with which the gas battery charged with oxygen and hydrogen would exhibit static effects; and, *secondly*, whether, when it was charged with gases which do not enter into chemical combination, *any* signs of tension could be elicited.

38. By careful arrangement I obtained an attraction in the gold leaves of the electroscope (fig. 6) with a series of nine pairs; with twelve or fourteen, the effects are very distinct, and required no very delicate manipulation. The series of forty pairs was then charged with oxygen and nitrogen; at first the electroscope was affected, and chemical action as well as dynamic effects were obtained with the closed circuit; these, however, were evidently due to impurities in the nitrogen (this gas was obtained by burning phosphorus in common air; the oxygen by electrolysis): after keeping the circuit closed for two days, these effects ceased, when not the slightest static, chemical or dynamic actions could be detected; the volumes of the gases remaining perfectly stationary, whether the circuit was closed or open.

39. From the preceding experiments we learn that, when

* Philosophical Transactions, 1843, plate v. fig. 1 [or Phil. Mag. S. 3, vol. xxiv. p. 270].

the gases used in this battery are oxygen and hydrogen (which will from their affinity for each other enter into chemical combination), a series of ten or twelve is sufficient to develope static effects; but that, when charged with gases which have not this power, viz. oxygen and nitrogen, even a series of forty does not affect the electroscope.

40. The advantage of using oxygen and hydrogen is, that although the hydrogen is slightly absorbed by local action with the atmospheric air in the solution, action does not take place in the oxygen tubes until electrolysis commences; and we have thus the most correct means of defining our results.

41. With hydrogen and chlorine, signs of tension were obtained with a series of six pairs of cells.

42. With chlorine in a single tube, and amalgamated zinc as the positive element, a combination by which, as Grove has shown[*], one pair will decompose water, a series of two pairs affected the electroscope.

43. Having thus elicited that for the purpose of obtaining static effects with a gas battery, it is indispensable that the two gases employed be capable of entering into chemical combination with each other, and having found that the higher the state of their mutual chemical affinities, the less was the number of series required to produce static effects, my attention naturally reverted to the older forms of the voltaic battery. I allude to those having two metallic elements excited by acid solutions.

44. Ten of the glass cells of the water battery (5.) were filled with dilute sulphuric acid. In each cell I placed a small porous earthenware vessel, also filled with the same solution. The metallic elements in this arrangement were amalgamated zinc and platinum; the latter being placed in the porous vessels, each cell was carefully insulated. This arrangement affected the leaves of the electroscope (fig. 6), while it required sixteen series of the water battery (5.) to produce the same effect.

45. The dilute acid was then removed from the porous vessels, and these were refilled with strong nitric acid, forming the well-known nitric acid battery of Grove. Three series of this arrangement affected the electroscope, and with the assistance of a Zamboni's pile, *distinct signs of tension could be elicited from a single pair*; the platinum showing vitreous and the zinc resinous electricity.

From the preceding experiments it appears, that to obtain evidence of tension, the principal requisite is good insulation,

* Philosophical Transactions, 1843, p. 103, Note [Phil. Mag. S. 3. vol. xxiv. p. 352 note].

and that this condition being fulfilled, the most energetic chemical battery will exhibit signs of tension before the completion of the circuit with a smaller series than that which is merely excited with rain water (44. 45.).

Conclusion.

The deductions which I make from the experiments described in this paper are as follows:—

1st. That the elements constituting the voltaic battery assume polar tension before the circuit is complete (10. 11. 15.) even in a single cell (45.) ; and that the existence of this polar state is demonstrated by the action on the electroscope being different at each terminal of the battery.

2nd. That this tension, when exalted by a series of pairs, is such, that a succession of sparks will pass between the terminals of the battery before their actual contact (16. 32.).

3rd. That these static effects precede, and are independent of, the completion of the voltaic circuit (10. 11. 17.), as well as of any *perceptible* development of chemical or dynamic action (18. 19.).

4th. That when the circuit is completed, whether by actual contact of the terminals, or merely by approximating them, so as to allow a succession of sparks, the dynamic effects on the galvanometer are the same (30.); each producing a steady deflection of the needle; consequently that the current, even when the circuit is closed, may be regarded as a series of discharges of electricity of tension, succeeding each other with infinite rapidity.

5th. That the rise of tension in a battery (the chemical affinities of its elements being feeble, as in the water battery) occupies a measurable portion of time (27.).

6th. That to produce *static* effects in the voltaic battery, it is indispensable that the elements should be such as can combine by their chemical affinities (34. 35.), that the higher those chemical affinities are exalted, the less is the number of elements required to exhibit the effects of tension (38. 39. 41. 42. 44 and 45.), and consequently, that the *static* effects elicited from a voltaic series are direct evidence of the first step towards chemical combination or dynamic action.

The chemical effects, when obtained in the generality of the experiments described in this communication, are of course feeble; but they are precisely the same in character as those exhibited by the more powerful voltaic combinations; and it may fairly be concluded that the rationale of each is the same, and that they only differ in the degree of action.

Received April 12, 1844.

Note.—I have recently constructed an instrument, by means of which the tension in a single series of the voltaic battery can with facility be tested without the aid of Zamboni's pile. Fig. 8 represents the electroscope, in the construction of which I was in a great measure indebted to an apparatus described by Dr. Hare[*]. A is a glass vessel, the stem of which is well-coated with lac; B, B', two copper wires passing through glass tubes and corks; D, D', gilt discs, each about two inches diameter, attached to the wires; P, a copper plate with a wire passing through a glass tube; to the end of the wire is attached a *narrow* strip of gold leaf, L. The discs must be adjusted with care, so as to allow the

Fig. 8.

leaf to be equidistant from each. If B is connected by a wire attached to the platinum, and B' by another wire attached to the zinc of a single cell of the nitric acid battery, insulated on a plate of lac, and an excited glass rod is approximated very gradually towards the plate P, the gold leaf will be attracted to B', or the disc attached to the zinc; and if excited resin is approximated in a similar manner, the leaf is then attracted to B, or the disc attached to the platinum. By means of this instrument, my friend, the Rev. Charles Pritchard, obtained signs of tension in a single cell excited by dilute sulphuric acid with platinum and zinc. This experiment I subsequently verified, and obtained similar results with single cells of other usual arrangements of the voltaic battery; but in all the experiments I made, the higher the chemical affinities of the elements used, the greater was the development of the evidence of tension.

March 1844.
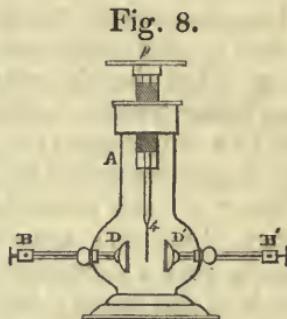
XLIX. *Proceedings of Learned Societies.*

ROYAL ASTRONOMICAL SOCIETY.

April 12, 1844. THE following communications were read:—I. Additional Observations of Faye's Comet, made at the Observatory of Trinity College, Dublin, by Mr. C. Thompson, accompanied by an explanation of the method of observation and reduction. Communicated by Sir W. Hamilton. For these we refer to the Monthly Notices, vol. vi. p. 67.

[*] Silliman's Journal, vol. xxv.

II. Meridian Observations of the Moon and Moon-culminating Stars, made at Hamburg during the years 1838 and 1839. By C. Rumker, Esq. Communicated by Dr. Lee.

III. Elements of the Comet of Mauvais. By M. Götze. Communicated by Dr. Lee. These elements will be found in the Monthly Notices, vol. vi. p. 68.

IV. Observations of the Comet of Encke, made at the Royal Observatory of the Cape of Good Hope in May 1842. Communicated by Thomas Maclear, Esq. See Monthly Notices, as above.

V. An Account of the Erection of the Herschel Obelisk at the Cape of Good Hope, accompanied by the Report of Colonel Lewis, and a Plan of the same. By Thomas Maclear, Esq.

At the last meeting of the Committee appointed to superintend the erection of the Herschel Obelisk, held on the 2nd of March, 1842, it was resolved that a plan of the same, together with that part of Colonel Lewis's Report which refers to its construction and erection at Feldhausen, should be forwarded by Mr. Maclear to the Royal Astronomical Society of London, with a request of the Committee that the same might be published in the Memoirs of the Society.

Mr. Maclear's engagements did not give him the requisite leisure for complying with the request of the Committee, and for collecting the additional information respecting the history of the obelisk, till the end of the year 1843.

The following is an abstract of the explanation furnished by him:—

Sir John Herschel, during his residence at the Cape, was President of the South African Literary and Scientific Institution. When he was about to leave the colony, the members expressed a desire to present him with some token of remembrance; and, at a full meeting, a few days before his departure, a gold medal was presented, with the impress of the institution on one side and a suitable inscription on the reverse. The feelings excited on that interesting occasion strongly evinced how much the members regretted the loss of their president and their admiration of one whose talents place him so far above ordinary men, and whose private life was a pattern of every domestic virtue.

The sum subscribed having exceeded the expense of the medal, another subscription-list was opened with the intention of raising a fund for the purpose of placing a substantial structure on the site of the 20-feet reflector in the garden of Sir John's late residence at Feldhausen. The proposal was accordingly laid before Sir George Napier, who entered warmly into the project, and placed his name at the head of the list annexed to a handsome subscription. In the course of a few days the sum subscribed amounted to 190*l.*

At a general meeting, held on the 28th of November, 1838, the erection of the obelisk was finally determined on; and a committee was appointed to carry its erection into effect.

A fruitless attempt to procure a granite column at the Cape, of proper workmanship and within the resources of the Committee, led to the adoption of a suggestion, that one of Craigleith stone,

from the quarry near Edinburgh, might be obtained without diffi-culty and of superior finish. A resolution was accordingly passed by the Committee, which, together with a plan of the proposed obelisk, was forwarded to Professors Forbes and Henderson of Edin-burgh, with a request that those gentlemen would kindly undertake the necessary superintendence of the work ; a request which they acceded to with alacrity ; and the obelisk, in packing cases, arrived in Table Bay in the month of August 1841, where it was safely landed under the guidance of Colonel Lewis.

The following is the report of Colonel Lewis on the erection :—

" In excavating the foundation, which was of black sand, it was found necessary to go down 4 feet 10 inches to arrive at the iron-stone gravelly bed, the substratum of the country about Feldhausen. The masonry foundation was formed of concrete, built up in courses of 12 or 14 inches, and composed of iron-stone, gravel, and lime-mortar, well grouted together. On this masonry bed a granite plat-form 9 feet 6 inches square was laid, and the small column fixed by Sir John Herschel on the site of the 20-feet reflector. This mark was removed for a few days, in order to bring the masonry founda-tion to a proper height, but the mark was relaid with mathematical correctness by Lieut. Laffau, Royal Engineers.

" Previously, however, to relaying the Herschel mark, the sugges-tion of the committee of construction was adopted of placing under it several silver and copper coins, a few inscription medals, and medals of the South African Institution, struck in silver for the oc-casion ; and on the obverse were engraved some notices, statistical and geographical, of the colony ; the discoveries of Capt. Ross in the South Polar Regions in 1841 ; and the operation of remeasuring the arc of the meridian in 1842. These subjects were beautifully executed by Mr. Piazzi Smyth, assistant-astronomer, and hermeti-cally sealed in glass bottles. Also there were deposited a map of the colony and engravings of nebulæ observed at Slough from 1825 to 1833, by Sir John Herschel, and a plan of Mr. Maclear's triangu-lation connecting the site of Feldhausen with the Royal Observa-tory and the site of Lacaille's observatory, in Strand Street, Cape Town.

" The bottle was carefully fixed in a block of teak-wood, scooped out on purpose.

" When the granite platform was brought to its level, and the Herschel mark refixed and filled in with cement, it was necessary to erect heavy shears of large spars, to place the stones of the obelisk composed of large blocks of Craigleith stone, some weighing two tons, sent from Scotland by Professors Forbes and Henderson, who kindly took this charge. This was accomplished with some trouble and expense, and the base of the obelisk was laid with the faces cor-responding with the four cardinal points. The whole was completed on the 15th of February, 1842, in presence of some of the Commit-tee and several of the subscribers and friends of Sir John Herschel, who attended on the occasion of placing the top stone of the obelisk.

" The obelisk has the base 6 feet square by 6 feet in height, and the pyramidal part stands 12 feet above the base. On the east face

is an opening showing the Herschel mark, designating the site of the 20-feet reflector. The opening will be closed with a bronze plate, containing the inscription of the purpose for which the obelisk is erected."

VI. On Loud Beats of Clocks used in Observatories. By J. S. Eiffe, Esq.

This paper gives an explanation of a simple and easily applied method of obtaining very loud beats for the astronomical clock. The author alludes to the difficulty which has been hitherto experienced in obtaining any sound in the beat of astronomical clocks at all approaching that which is necessary for distinctness under ordinary circumstances. He then adverts to the necessary irregularities in the clocks of the ordinary construction, arising from the largeness and imperfection of the workmanship of the brass-work, and hints at a method which he has devised, whereby the present large circle of seconds can be retained with a small and delicate movement, such as those used in marine chronometers, and without any intermediate wheel.

But without a method to enable the observer to increase at pleasure the loudness of the beats, it is manifest that the beats of such a clock would not be loud enough for the ordinary purposes of an observatory; and the author's invention, described in the present paper, is intended to supply this deficiency by the introduction of an apparatus to produce loud beats simultaneously with the escape of the seconds' wheel from one pallet to the other, which is totally independent of the size and weight of metal of the works of the clock. The mode of constructing the apparatus is as follows :—

Two pieces of thin brass are placed at the sides of the framework of the clock, in length the same as the space between the pillars; in width, about two inches or more at pleasure; these pieces of brass are placed horizontally, at about the same altitude from the base as the axis of the escape-wheel pinion, and at right angles to it, or nearly so. They should be made of such a size as would ensure a sound, distinct, sharp, and short. The little tables can be made to any size, as is mentioned above. Upon these tables or plates two hammers ply, supported by arbors at the same elevation as all the others. The pivots should be made small for easy motion. The hammers are intended to beat upon the middle of each brass table simultaneously with the drop proper of the escape-wheel; through the agency of the pendulum, they are lifted alternately by the heels of the anchors of the pallets, assisted by a passing spring similar to that used in the chronometer escapement. It has just been observed, that the arbors which support those little hammers are placed at the same elevation from the base of the brass framework of the clock as the escape-wheel arbor, but at the sides, and as near to the edge as possible. About the centre, or midway between them, are affixed brass collets, about 1-8 of an inch in thickness, and 1-4 of an inch in diameter. Two slender pieces of spring are secured to the collets by screws passing through square holes formed longitudinally, to secure power of adjustment for bringing the arms into proper contact with the anchor of the pallets. The little ham-

mers beat upon the plates or tables at one end, and at the other the lifting action takes place, assisted by the passing spring. The strokes upon these brass tables have a peculiar sharpness of tone, which can be accounted for in some measure, when it is considered that they are very different from the sounds produced by the teeth of the wheel itself; in the dead-beat escapement the teeth have a sliding motion in the moment of drop, but not impulse, for it is well known that that is subsequent to the sound. The exertions to obtain sound have for some years been considerable; but the result has been a poor recompense, and unimportant after all. Thus then, and by such application, simple and easy to be understood, is it proposed to obtain sound, so loud as to be *distinct in the stormiest night*; but as the constant connexion of such apparatus would neither be desirable as concerns the action of the clock, nor pleasant to the ear as a companion, a mode has been introduced of readily detaching it altogether. By a certain method, which shall be explained, the hammers are raised from the tables at one end, and the arms at the other entirely disengaged from the anchor of the pallet, without the least inconvenience or disturbing action to the clock itself. This is very pleasing, and greatly to be desired, and perhaps may be considered of nearly equal importance with the invention itself by which a loud beat is obtained. The apparatus within is immediately, and at pleasure, acted upon through the agency of a bolt, which is placed vertically, immediately over the 60 minutes, or about two inches back, sufficiently long to reach a spring of hard brass, which is about half an inch wide, and which passes transversely over the frame-work of the clock, and is fixed securely to the back board of the clock-case. Now the mode in which the spring unites its action with the rest of the apparatus is by slight cross-bars, which extend to the extremities of the sides of the frame, so that the ends are immediately over the hammers, with which they are connected by silk threads. Therefore, by pressing down the bolt before named, the hammers are allowed to fall into action, and do their duty simultaneously with the teeth of the wheel upon the pallets. While the little hammers are in action, the teeth of the wheel are no longer heard, and the motion and view of the seconds' hand, as well as the ear of the inquirer, will satisfy him as to the usefulness of the invention.

In conclusion, and to endeavour to dispel any doubts that may arise as to the probable advantages to be obtained by such assistance to the observatory, the opinion and satisfaction expressed by word and by letter, on those advantages, by the distinguished individual who is at the head of our national observatory, will, it cannot be doubted, be interesting to this learned Society. The Astronomer Royal declares by letter, that he has examined the plan, and is enabled to say that it answers completely for its proposed purpose ; and that it appears likely to be very useful; moreover, that the rate of the clock will not necessarily be disturbed during the time of its connexion—though that will greatly depend on certain conditions.

May 10.—The following communications were read :—

I. The Right Ascensions of the Principal Fixed Stars, deduced from Observations made at the Observatory, Cape of Good Hope,

in the years 1832 and 1833. By Thomas Henderson, F.R.SS. L. and E., Professor of Practical Astronomy in the University of Edinburgh.

This catalogue contains the right ascensions of 174 principal fixed stars, reduced to January 1, 1833; being the same stars of which the declinations, observed by Mr. Henderson at the Cape, are published in vol. x. of the Memoirs, with the addition of θ^2 Eridani, α Persei, and θ Ursæ Majoris. The observations of right ascension (with the exception of a small number) were made by the assistant-astronomer, Lieutenant William Meadows, R.N. The transit instrument, by Dollond, is 9 feet $9\frac{1}{2}$ inches in focal length, with an object-glass of 5 inches in diameter. The magnifying powers employed were 88 and 132; chiefly the latter. The clock was by Hardy, having one of his scapements, and a mercurial compensation pendulum. During the period of the observations its rate was as uniform as the rates of most transit-clocks in use at observatories. The reductions of the right ascensions to January 1, 1833 (when the sun's mean longitude was 281°), were computed from the tables in the supplements to the Nautical Almanac for 1832 and 1833. The coefficient of aberration was assumed to be 20″·5; and the proper motions in right ascension were taken into account, when they appeared to amount annually to 0ˢ·01 of time. The right ascensions given in the catalogue are the means of all the determinations from observations made above and below the pole without distinction. The annual precessions are calculated for the beginning of 1833, from the *Tabulæ Regiomontanæ*. The right ascensions of the present catalogue are compared with those of the same stars in the catalogues of Lacaille, Bradley, Piazzi, Rumker, Johnson and Pond (all reduced to the same epoch), and the differences exhibit the effect of the proper motion in right ascension for each of the stars respectively.

From these differences, combined with the differences of declination given in Mr. Henderson's former catalogue above referred to, the annual proper motions of the stars in the present catalogue are obtained. A table is given, which contains such of them as appear to have proper motions not less than 0″·1 of arc; they amount to 35. The following are those whose annual proper motions exceed 0″·4.

Star.	Total annual proper motion.	Annual proper motion in right ascension (in time).	Annual proper motion in decl.
	″	sec.	″
α Centauri	3·58	−0·464	+0·82
β Hydri	2·17	+0·716	+0·29
α Canis Majoris	1·34	−0·034	−1·25
θ Centauri	0·79	−0·047	−0·54
γ Pavonis	0·74	+0·013	+0·73
ϵ Scorpii	0·69	−0·050	−0·29
$\varkappa$ Phœnicis	0·47	+0·014	−0·44
β Trianguli Australis.	0·44	−0·030	−0·40

X 2

II. Observations on the Appearance of the Comet of 1843, made at Cape Coast Castle, on the Coast of Africa. By G. Maclean, Esq., President of the Colony. Communicated by Captain Beaufort, R.N., Hydrographer to the Admiralty*.

The comet was first seen at Cape Coast Castle on the evening of Friday, the 3rd of March, at about a quarter to seven. Part of its tail only was then visible, bearing W.S.W., and making an angle of about 70° with the horizon, towards the south. It was of the same brightness throughout, and its breadth, which was little more than a degree, so far as it could be seen on account of both extremities being concealed by clouds, was also uniform.

March 4.—This evening the whole of the comet was visible, although no nucleus could be distinguished. Its head, or what appeared to be so, almost touched the horizon, near the star iota in the tail of the whale; and its tail extended about 22° from that point in the direction of the constellation Columba Noachi.

March 5.—Several glimpses of what appeared to be a nucleus were perceptible through the telescope of a theodolite. It appeared as a bright point, of the colour of Venus, but exceedingly small. Being invisible through the telescope of the sextant, distances could not be ascertained with any degree of precision.

March 6.—The appearance of the comet was the same as the preceding evening. March 7, the brightness of the head and the length of the tail were much increased, the latter extending upwards of 34° in the direction of the constellation Lepus. Several stars were visible to the naked eye through the tail. On the 9th and 10th, the appearance of the comet was much the same as on the 7th; on the 17th it was visible, but the nucleus was very indistinct. The tail extended about 43° in the direction of Sirius.

March 19.—This night was clear, and the outline of the comet very plainly marked. The bright spot or condensation in its head was distinctly perceptible to the naked eye. On the 22nd, although the sky was very clear, the nucleus was with difficulty perceptible, from which it appeared that the comet was increasing its distance from us with immense rapidity. The tail terminated midway between the stars ζ, η and θ Leporis, and ϰ Orionis.

After this time the comet decreased in brightness and size every night. On the 23rd its tail was about 38° in length; on the 26th about 35°, reaching a little past ϰ Orionis. Through ordinary land-glasses it still appeared as if there was a condensation of brighter matter in the centre of the head. The comet continued visible on clear nights till about the 10th or 12th of April, appearing as a thin haze; but after the 1st no observations could be taken with the sextant.

The following are the observed distances of the comet from Sirius and Aldebaran. They are given without any correction, just as they were read off from the sextant; and the observations were made about seven in the evening.

* For other observations of the comet of 1843 see Phil. Mag. S. 3. vol. xxiv. p. 522.

| Day. | Distance from | | Day. | Distance from | |
	Sirius.	Aldebaran.		Sirius.	Aldebaran.
1843. March 3	94 15	69 45	1843. March 19	54 24	34 7
5	90 35	67 12	22	50 0	30 42
6	87 18	63 51	23	48 22	29 51
7	83 48	60 36	26	44 53	26 40
9	77 37	54 30	27	43 39	25 40
10	73 50	51 57	28	42 45	24 59
17	58 50	37 52	29	40 58	24 28
18	56 16	36 50	April 1	38 8	22 58

III. Extract of a Letter from J. R. Crowe, Esq., British Consul-General of Norway, to Dr. Lee, dated Alten, February 22, 1844. Communicated by Dr. Lee.

" The observatory at Alten, as you are aware, is the most northern in the world. We shall very shortly commence the series of preparatory transit observations you suggest. We had intended to have done so before ; but the cold, since the sun revisited us on the 2nd instant, has been so intense that we have not been able ; the eye was no sooner brought near the glass than the latter was covered with a coating of ice. It was even dangerous to touch the metal. The thermometer has been varying from 24° to 28° below zero of Celsius, that is, from 12° to 20° below the zero of Fahrenheit, or from 44° to 52° below the freezing point . . . . It will be interesting to see what the minimum thermometer will exhibit at the top of the Storvandsfeldt by the end of next month. We shall then attempt the ascent, as by that time the surface of the snow becomes so hard as to bear walking upon with impunity."

IV. Elliptic Elements of Bremicker's Comet. Computed by Mr. William Götze. Communicated by Mr. Rumker, and also by Professor Schumacher. Stated in the Society's Monthly Notices, vol. vi. p. 78.

V. Elements of the Comet of Faye, 1843–4. Corrected by the Observations at Greenwich on Jan. 22, Feb. 19, 20 and 22 ; Paris, on Dec. 2 ; Cambridge, on Dec. 8 and 16 ; at Berlin, on Dec. 17 ; Hamburg, on Jan. 23 ; and Starfield, on January 13, and Feb. 19 and 22. By J. R. Hind, Esq. Communicated by the Rev. R. Main.

Epoch, 1844, Jan. 1·0, Greenwich mean time.

Mean longitude........................ 60 30 0·2
Longitude of perihelion on orbit ... 50 34 19·0 Mean equinox.
Argument of latitude in perihelion. 201 24 13·1
∴ ☊ .... 209 10 5·9
Inclination .... 11 21 2·8
Angle of eccentricity................. 33 30 52·3
Mean daily sidereal motion 477″·56329
Log. semi-axis major ...... 0·5806505
Period of sidereal revolution, 7^{yrs}·43
Direct.

The elements represent the Greenwich observation on the 20th of February with errors of $-2''\cdot 3$ in longitude and $-11''\cdot 0$ in latitude.

The observation at Berlin on Dec. 17 gives $+8''\cdot 4$ in longitude and $+24''\cdot 1$ in latitude.

The period agrees exactly with that found by Plantamour, and very nearly with the results of Le Jeune, Faye, &c.[*]

June 14.—The following communications were read :—

I. Some remarks on the Telescopic Appearance of the Moon, accompanying a Model and a Drawing of a Portion of her Surface. By James Nasmyth, Esq.

The model and drawing submitted to the Society by the author, represent a portion of the moon's surface of 190 by 160 miles, situated in the upper part of her left limb as seen in an inverting telescope, having in the centre the large crater marked in the Berlin chart No. 29, and named "Maurolicus."

The scale of the drawing is one-eighth of an inch to a mile. The telescopes employed were two Newtonian reflectors, one of $8\frac{3}{4}$ inches aperture, and 9 feet focal length, and the other of 12 inches aperture, and 13 feet focal length, the powers employed being 240 and 360. The author has, for the last four years, confined his attention almost exclusively to the nature and structure of the lunar disc, and he selected the portion above mentioned as a subject for a model by reason of its comprising in a small space most of the chief features which so remarkably distinguish her surface.

The model was constructed with a view of illustrating the close relationship which appears to exist between the structure of the lunar surface and that of a considerable portion of the earth, in regard to the similarity in the results of vast volcanic action.

The author, in reference to the nature of the peculiarities of the surface of the moon, first remarks on the *vast size* of the lunar craters as compared with those on the surface of the earth. Of these there exist some of the enormous magnitude of 150 miles in diameter, besides other circular formations, such as the "Mare Serenitatis," and "Mare Crisium," which are from 200 to 300 miles in diameter, and which evidently owe their form to volcanic action of prodigious central energy. This enormous effect, compared with that of volcanic agency on the earth's surface, will appear less surprising when we consider that the mass of the moon is scarcely the $\frac{1}{70}$th part of that of the earth, and that, consequently, the weight of the materials acted on by the volcanic force is diminished very considerably compared with bodies on the earth's surface : the probable want of atmospheric resistance will also assist in accounting for the immensely greater effects produced. The beautiful and almost perfectly circular form of the majority of the lunar craters may be due to the absence of wind or other disturbing causes, permitting the discharged materials to perform the course due to the impulse comparatively free from all impediment.

There are several portions of the moon's surface which indicate that considerable *time* has elapsed between the formation of one

[*] See also our preceding volume, p. 519, and former reports of proceedings of the Astronomical Society in the present volume.—Edit.

crater and that of another, this being proved by the fact of the circular mound of the one overlaying that of the other. [The author here introduces a pen sketch illustrating this remark.]

Next to the circular form of the craters, the author considers that there is no feature more striking than the small cones or mounds which we observe in the centre of most of the craters. These he considers to be the result of the last expiring efforts of the volcanic action, as we find it to be the case in Vesuvius and other terrestrial volcanoes. Other cases exist in which there is no such central cone; but these may have resulted from the more sudden termination of the volcanic action which had permitted the fluid sooner to float across the bottom of the crater, and to form that plain, smooth surface which may be seen in a few cases. One has been, however, observed by the author in the upper part of the right limb of the moon, in which the lava had apparently kept flowing up so gently to the last as to leave the crater brim-full. [This is illustrated by a pen drawing.] The ruts or channels which may be distinctly observed in the sides or banks of the outside circular mounds, and which frequently extend to a considerable distance, prove that the matter discharged has not been entirely of a solid nature. Blocks of solid materials also appear to have been discharged with vast force and in vast quantity. They may, in many cases, be observed lying about the bases of the larger craters, where the surface is rendered quite rough by the quantity of such detached fragments.

The last peculiarity adverted to by the author consists in the bright lines which generally converge to a centre, and in which we frequently find a crater of very considerable magnitude. " Tycho," " Copernicus," and " Kepler," are remarkable examples of this appearance. The material of these bright lines is evidently of a much more reflective nature than the contiguous or general surface of the moon, and in most cases the interior of the crater to which they converge is equally resplendent. The author considers them to be derived from the same original cause which produced the central volcano from which they appear to diverge. It appears to him that they are produced by the flow of the molten lava through the vast cracks resulting from the great primary upheaving action which had burst upwards the solid surface of the moon, cracking it as a pane of glass does when broken by any pointed object. The centre of disruption has evidently been under the great central volcano. The cracks have diverged on all sides from this centre of action; and the molten lava immediately flowing up would come forth in greatest quantity from the centre, and there result in and produce the great crater, while the radiating cracks would yield smaller portions *simultaneously* all along their course, however extended.

II. Observations of the Solar Eclipse of 1843, December 21, made at the Observatory of Trevandrum. By John Caldecott, Esq.

The account of the eclipse is communicated in a letter to Mr. Baily, dated February 21, but was unfortunately too late for the mail of that month. Mr. Caldecott having satisfied himself that the eclipse would be total, in latitude $11°\ 45'$ north, and longitude $5^h\ 3^m$ east, determined on proceeding to the neighbourhood of Tellicherry

(on the coast), for the purpose of observing it. It was necessary to be very precise in this calculation, and also to be very certain of the geographical position of the spot selected for observing the eclipse, on account of the almost exact correspondence in size of the sun's and the moon's discs. Mr. Caldecott, in the first place, proceeded by sea to Mahe (a little to the south of Tellicherry), and thence up the Mahe river towards its source, and having, by a careful survey of the river, ascertained its position with respect to Mahe (whose latitude and longitude he had previously ascertained), he selected a clear and open space on a rising ground, about three miles north of it, and there pitched his tent at midnight of December 20. He had brought with him an excellent 30-inch achromatic telescope, having an object glass of $2\frac{3}{4}$ inches aperture, with an eyepiece giving a magnifying power of about 50, protected by a smoked glass gradually increasing in intensity from one end to the other, and sliding easily in a groove, wherewith to observe the eclipse; a sextant and artificial horizon, with a good pocket watch for time; and two native assistants, with an actinometer for observing the rate of decrease of solar radiation.

At daylight on the 21st (civil reckoning), the author mounted his telescope on a stand having a very smooth parallactic motion, and made all the necessary preparations, taking a set of altitudes of the sun's lower limb at about 7^{h} A.M., and stationing the native observers at the actinometer. He began watching the point of the sun's disc, where the edge of the moon's limb was to impinge at $7^{\text{h}} 35^{\text{m}}$, the sky being at this time quite clear in every direction, with the exception of a few light fleecy clouds which hung about the sun, but which cleared away before the eclipse became total. The observations that follow were taken almost verbatim from the author's notes written during the progress of the eclipse. At $7^{\text{h}} 38^{\text{m}} 49^{\text{s}}$, corrected mean solar time, civil reckoning, the contact was evident, though the real first contact was probably two or three seconds earlier. With one digit eclipsed the border of the moon was fully as tremulous as that of the sun, and no hollows or coruscations were to be seen. No appearance of any *inherently* bright spots about the moon's disc was observed about this time, and no spots on the sun. The cusps were perfectly sharp and distinct; the colour of the sun a pearly white, inclining to a metallic or silvery white. With eleven digits eclipsed there was no appearance of a corona, but shortly after three protuberances were observed near the southern cusp, and none on the other. [This is illustrated by a drawing.] As the conjunction of the discs approached, Mr. Caldecott watched very closely for the "beads," but saw none until the very instant of conjunction, when the whole of the southern limb of the sun broke up into most beautiful beads of silvery light, with a clear but very fine line of light joining them, and extending somewhat beyond them, until lost in the corona of the opposite limb. [This is illustrated by a drawing.] These beads for the second or two of their remaining, appeared to form, break up, and form again, and resembled globules of mercury of different sizes in a state of violent agitation.

They never entirely disappeared, so that the sun wanted the *small-*

est imaginable quantity of being *totally* eclipsed here. The corona was very faint and of a greenish-yellow colour, occasioned doubtless by seeing it through a very thin film of smoke, for the total removal of the darkening shade had not been provided for. It was never quite dark, or too dark to read off the actinometer : Venus, Saturn, and Arcturus were seen by the author's servants, who had been instructed to watch for stars. Mr. Caldecott saw nothing of the luminous protuberances noticed by Mr. Baily and Mr. Airy*, but the greatest obscuration was so momentary in this eclipse, that there was no time for the eye to give more than a glance round the disc before the sun and moon were again separating, and strong light was restored. Shortly after 9ʰ A.M. the author took another set of altitudes of the upper limb of the sun, and the appearances during the separation of the two bodies were observed every few minutes, but nothing worthy of remark was seen. The actinometer readings and the resulting radiation are given in an accompanying table, together with some occasional readings of a thermometer taken at irregular intervals during the progress of the eclipse. The impression on Mr. Caldecott's mind is, that the beads are the effect of the incessantly varying refraction of the atmosphere on the two limbs, causing each of them to have an apparently dancing or *bubbling* motion, and thereby producing the appearance of spots of light, with intermediate dark spaces or bands, after the body of the sun is in reality covered by that of the moon, just as a planet or bright star when dipping into the horizon is frequently seen on a fair, clear night at sea to disappear and reappear repeatedly. This appearance of beads he believes to have been observed hitherto only at the eastern and western limbs, and, as these approach to and recede from each other *gradually*, the dark spaces or lines are more distinct for the moment of their appearance than in this case, where the limbs just grazed along each other, as it were, and in which, therefore, the bright spots assumed more of the form of globules, altering their shape, size, and position, as the two bodies moved past each other, with inconceivable rapidity.

The corrected observed time of the termination of the eclipse was 10ʰ 28ᵐ 51ˢ·5 A.M.

The longitude of the place of observation is 5ʰ 3ᵐ 4ˢ East.

The latitude.................................... 11° 44′ 38″ North.

III. Sextant Measures of the Sun at the Eclipse of the Sun on the 21st December, 1843. By Captain Sir Edward Belcher, R.N. Communicated by Captain Beaufort, R.N.

The observations consist of nineteen measures of the breadth of the illuminated portion of the sun during the progress of the eclipse. The longitude of the place of observation was 124° 12′ 30″ east, the latitude 24° 21′ 20″ north.

IV. On a Graphical Method of Predicting Occultations. By J. I. Waterston, Esq. Communicated by Captain Beaufort, R.N.

The following is the author's account of his method :—

" The following is a description of a graphical method of predicting

* See Phil. Mag. S. 3. vol. xxii. p. 386, 391.

occultations, which I have found useful in drawing the attention of some of my pupils to this valuable method of determining meridian distance.

" The prediction of the time is almost an essential preliminary to the observation, and the computation required for this, in the usual way, is such as to put it altogether beyond the reach of many who would otherwise be perfectly able and willing to make the observation, and who have many valuable opportunities in the course of their profession of improving geography by such means. The method is simply delineating the essential points in the orthographical projection of the moon's motion and that of the observer in a plane perpendicular to the direction of the star. This is simplified to the utmost by means of scales, and I find that, with a little practice, my pupils have no difficulty in predicting the time of observation to 1 or $1\frac{1}{2}$ minute, and this without much care being taken in the drawing. From several occultations, which I predicted and observed here, and from numerous examples taken from the Astronomical Society's Transactions, I find that, with ordinary care, the predicted time may be depended upon within one minute, and that the time occupied in doing so varies from 10 to 15 minutes. As this is sufficiently near for all the wants of the observer, it may, perhaps, serve to supersede the method of computation, which, in its simplest form, is an irksome task, and probably tends to make occultations less frequently observed than they otherwise might be.

" With officers duly initiated, and observatories at work, observing all that are visible as a regular part of their duty, the most valuable results to geography might be anticipated.

" I have appended a rule for computing the moon's right ascension and the consequent error of assumed longitude, which is derived from the same method of orthographical projection. It is rigidly correct in principle (with the exception of taking small arcs instead of their sines, which in no case affects the result in any sensible degree), and the results of computation may be depended upon within a hundredth part of a second of right ascension if the data are correct, and a considerable error in the estimated longitude, will, I think, be found to have as little injurious effect as in any other method of computation."

" Bombay, May 1, 1844."

V. Some Remarks on the Great Comet of 1843, as seen in the neighbourhood of Paramatta, New South Wales. By the Rev. W. B. Clarke. Communicated by Sir John Herschel[*].

The principal point to which the author directed his attention, during the time of the visibility of the comet, was the fact of the existence of a small train of light inclined at a small angle to the large train.

His own observations were corroborated by those of the Bishop of Australia, who had made distinct notes of its appearance. His lordship says :—

" On the evenings of Thursday the 2nd instant, and again on Saturday the 4th, my attention was drawn to the remarkable spec-

[*] See *ante*, p. 308.

tacle of a definite portion of the tail being deflected from the axis, or direction in which the general body of light continued to proceed. Perhaps about one-sixth of the train might be thus drawn aside from that which may be termed the natural direction, so as to form therewith, at the point of separation, an angle which I should calculate to be about three degress. . . . Five-sixths of the whole body of light continued without interruption in the ordinary direction, the remainder deviating from it in the manner here stated."

VI. Observations made at the Observatory of Hamburg. By C. Rümker, Esq. Communicated by Dr. Lee.

The observations consist of:—

1. Continuation of the Meridian Observations of the Moon from 1840, August 7, to 1841, February 12.

2. Observations of the Eclipse of the Moon of 1844, May 31.

3. Observations of Pallas and Ceres during their opposition, May 1844.

VII. Scheme of Planetary Elements. By S. M. Drach, Esq.

L. *Intelligence and Miscellaneous Articles.*

PREPARATION OF THE PURPLE POWDER OF CASSIUS.

M. FIGUIER gives the following as a certain process for preparing the above-named compound:—dissolve 300 grains of gold in five times their weight of aqua regia, prepared from four parts of hydrochloric acid and one part of nitric acid; evaporate the solution almost to dryness; this evaporation is requisite to get rid of the acid. The chloride of gold being redissolved in water and filtered, the solution is to be diluted till it measures 26 ounces; fragments of granulated tin are then to be put into it, which becomes turbid and brown in a few minutes; its tint gradually becomes deeper, and at the end of a quarter of an hour it assumes a fine purple colour, the precipitate is deposited, and it remains only to collect it on a filter.

It sometimes happens, and especially when large quantities are operated on, that the precipitate does not separate, but remains in the liquid, to which it gives a deep purple colour; in this case it is merely requisite to heat the liquid slightly and to add a little common salt; the product then immediately separates.

When the liquid holding the purple powder in suspension is decanted to separate the excess of metallic tin, care must be taken that no particles of tin, which remain at the bottom of the vessel in the state of a black powder, are poured off with it; it is proper to allow the liquor to settle for some time and afterwards to decant it; this operation should be repeated three or four times.—*Ann. de Ch. et de Phys.*, Juillet 1844.

ON THE OXIDES AND SOME OTHER COMPOUNDS OF GOLD. BY M. FIGUIER.

In preceding Numbers we have stated the methods employed by M. Figuier in preparing the protoxide of gold, and given his account of its properties, we now proceed to the

Auric Acid.—This is prepared by two processes:—1st, by treating

chloride of gold at a boiling heat with bicarbonate of potash, till effervescence ceases : 2nd, by precipitation with magnesia, as recommended by Pelletier. The first of these processes, as it is generally effected in the laboratory, yields but a very small quantity of the oxide, for an excess of the alkaline salt readily redissolves the oxide of gold precipitated, forming a double chloride of gold and potassium and aurate of potash. The magnesia process succeeds much better, though it is tedious, and a considerable quantity of the metal is converted into chloride of gold and magnesium.

M. Figuier prefers the following process :—precipitate the solution of gold by carbonate of soda, and convert afterwards the small quantity of gold which remains in the liquors, into aurate of soda, which is to be afterwards decomposed by sulphuric acid, so as to set the auric acid free : the following is M. Figuier's description of the process :—

Evaporate the common solution of gold nearly to dryness, redissolving with a few drops of aqua regia the light deposit of protochloride of gold, which always remains by the action of the heat. Dissolve the chloride in water, and saturate the brown solution which remains very accurately with carbonate of soda, either dissolved or solid, and as free as possible from chloride, to prevent the formation of the double chloride of sodium and gold. The liquor is to be made to boil, preserving its neutrality very exactly. It is to be boiled for half an hour, or rather till heat ceases to occasion precipitation.

By this process the greater part of the gold is precipitated of a fawn colour. In operating on about 460 grains of gold, only about 63 remained in the liquor separated from the precipitate ; and this may be converted into auric acid by a process stated by M. Figuier.

The teroxide of gold prepared by means of magnesia, contains after drying *in vacuo* or by exposure to the air, 29 per cent. or 10 equivalents of water ; and this is also the case when it is prepared by precipitating the aurate of potash with nitric acid ; but the teroxide obtained by means of carbonate of soda contains only 24 per cent. or 8 equivalents of water. If it be attempted to deprive these oxides of the water which they contain by means of the water-bath, they do not lose it without a portion being reduced by the heat ; so that when auric acid is required quite free from reduced gold, it must be dried either *in vacuo* or by exposure to the air ; this oxide is reduced at 473° F.; at common temperatures hydrogen has no effect upon it, but when the temperature is slightly raised, it reduces it very quickly ; when a current of this gas, dried by chloride of calcium is directed upon auric acid contained in a small glass tube, and a part of the oxide is very gently heated, it burns directly and vividly, and a beautiful flame, which is at first green and then red, pervades the train of oxide. The gold remains in the metallic state.

Iodine, when moderately heated, in the presence of water with auric acid, yields a milky liquid, which soon deposits a bright yellow powder. If the mixture be boiled, the iodide is reduced and deposits the gold ; M. Figuier has not analysed this compound, but supposes it to be periodide of gold, which has not hitherto been isolated. Alcohol when cold has no action on auric acid, obtained by the satu-

ration of aurate of potash, but at high temperature it quickly reduces it to the metallic state.

Auric acid assumes different colours according to its condition as a hydrate. Thus prepared with aurate of barytes, or with magnesia, or carbonate of soda, it is of an ochre-brown; when precipitated cold from chloride of gold by potash not in excess, it is canary-yellow; precipitated from aurate of potash by a weak acid, as the carbonic, or by very dilute sulphuric or nitric acid, it is also canary-yellow. If the acid be concentrated, it is of an olive colour, and seems of so deep a green that it appears black. Nitric and sulphuric acids of a common degree of dilution precipitate it from the solution of aurate of potash of a deep yellow colour ; the differences of colour depend in all cases on the proportion of water contained in these oxides, for they are all of similar composition, containing 10 per cent. of oxygen.

Aurate of Potash.—In order that auric acid may dissolve readily in potash, it must be obtained in the gelatinous state, by the decomposition of an aurate; the solution is with difficulty evaporated so as to prevent the precipitation of protoxide of gold; the only method by which Mr. Figuier could succeed in obtaining it free from protoxide was that of exposing very small quantities to spontaneous evaporation. This salt is of a greenish-yellow colour, very soluble in water and in alcohol; the alcoholic solution suffers no change by exposure to the air ; but if it be heated to 112° F., the whole of the gold is immediately precipitated of a brown colour.—*Ann. de Ch. et de Phys.*, Juillet 1844.

ON THE PREPARATION AND PROPERTIES OF CERTAIN CHLORATES. BY M. ALEXANDER WAECHTER.

Protochlorate of Mercury.—Recently precipitated protoxide of mercury was dissolved in chloric acid; the solution, evaporated over sulphuric acid, yielded fine prismatic crystals to the last drop; but they quickly lost their transparency and splendour, and became dull by exposure to the air. Water and alcohol readily dissolve the unaltered crystals, without leaving any residue ; they also dissolve those which are altered, but leave a small white residue, which becomes black by ebullition. If this salt be triturated with water and chloride of sodium, a white precipitate is obtained, and the supernatant liquor has a neutral reaction. The salt is therefore a neutral chlorate of the protoxide of mercury. If it be heated it loses oxygen at 482° F., and is converted into a mixture of binoxide and bichloride of mercury ; the latter may be separated by sublimation at 563° F., and the first remains if the temperature do not exceed 698°. If it be heated rapidly, the decomposition takes place suddenly, and chlorine is one of the products of the decomposition.

When the solution of this salt is evaporated to dryness by the water-bath, and the crystalline mass remaining is treated with water, an insoluble modification is obtained, already described by Vauquelin, as a white insoluble powder; this salt, triturated with water and chloride of sodium, yielded also a white precipitate of calomel, it was not therefore a basic salt, and it was completely dissolved in acetic acid. All the mercury was precipitated in the state of calomel from

the acetic solution, by hydrochloric acid; it yielded at a high temperature the same products as the soluble crystalline salt. The acid liquor with which it had been washed, deposited by evaporation calomel in a mammillated form, and chlorate of peroxide of mercury in crystals. When mixed with combustible bodies, protochlorate of mercury gave as loud an explosion as the chlorate of silver.

This salt consists of

Chloric acid	26·37
Protoxide of mercury . .	73·63
	100·

Perchlorate of Mercury.—By dissolving peroxide of mercury in chloric acid and evaporation by the water-bath, perchlorate of mercury was obtained in small tabular crystals. When these were heated they yielded at first water and afterwards oxygen; they are then converted into a mixture of peroxide and perchloride of mercury, the latter of which sublimes at 563° F. If the salt be rapidly heated, it decomposes suddenly, and chlorine is one of the products. When mixed with inflammable bodies, it usually inflames during the operation of mixing, but without detonation. Water decomposes it into an acidulous soluble salt and an insoluble basic salt. If it be triturated with chloride of sodium and a little water, it separates into red oxichloride of mercury, and a neutral solution remains.

By analysis this salt was found to consist of

Chloric acid	24·91
Binoxide of mercury. .	72·13
Water	2·96
	100·

Chlorate of Cadmium.—The solution of this salt, prepared by decomposing chlorate of barytes with sulphate of cadmium, yielded by evaporation over sulphuric acid, very deliquescent prismatic crystals extremely soluble in alcohol. This salt melts at 176° F., and yields water, oxygen and chlorine. When the heat has been sufficiently great, the residue is a gray fused mass, formed of a mixture of oxide and chloride of cadmium; if this residue be treated with water, it dissolves a portion of the chloride of cadmium; another portion remains and forms, by combining with the oxide of cadmium, a white insoluble basic chloride; the chloride of cadmium is so feebly combined with the oxide, that the carbonic acid of the air is sufficient to convert it into a mixture of carbonate and chloride of cadmium; and the latter may be dissolved out by water.

This salt was found to consist of

Chloric acid	47·90
Oxide of cadmium . .	40·56
Water.	11·45
	99·91

Chlorate of Bismuth.—The solution of oxide of bismuth in chloric acid decomposes by concentration *in vacuo*; it is then precipitated to the bottom of the vessel as basic chloride of bismuth, and hypochlorous acid is disengaged; on these accounts it was impossible to isolate the chlorate of this metal.—*Journ. de Pharm. et de Ch.*, Mai 1844.

DOUBLE SULPHATE OF PROTOXIDE AND SESQUIOXIDE OF IRON.
BY M. J. A. POUMAREDE.

When a mixture of two parts of protosulphate of iron and two parts of persulphate of iron, theoretically neutral, is treated with five or six parts of distilled water, it is found, after a contact of fifteen to twenty minutes, that the mixture becomes considerably warm, acquiring a temperature of 77° to 86° Fahr. above that of the surrounding air. In addition to this the two salts are found to dissolve entirely, yielding a perfectly bright brown liquor, which becomes a crystalline mass by proper evaporation.

There are two remarkable facts to be noticed in the reaction above described : first, the increased temperature of the mixture ; secondly, the solution of two salts, of limited solubility, to an enormous extent in a small quantity of water, and these are characteristics of chemical action ; the new salt crystallizes in long slender crystals ; they have a very pale green colour, a slightly styptic taste, and a very distinct sweet after-taste.

By long exposure to the air this salt is covered with subsulphate of peroxide ; but the tendency to peroxidize appears to be less than that of the protosalts of iron ; when heated it fuses readily, loses its water of crystallization, afterwards the acid of the persulphate, and at a still higher temperature it yields all the products obtained by decomposing the protosulphate ; water dissolves it in all proportions.

This salt was analysed by determining the increase of weight which it acquired by conversion into persulphate.—*Journ. de Pharm. et de Ch.*, Juin 1844.

METEOROLOGICAL OBSERVATIONS FOR AUGUST 1844.

Chiswick.— August 1. Cloudy: boisterous : clear. 2. Clear and very fine. 3. Showery: boisterous. 4. Dusky clouds : clear and fine. 5. Cloudy and fine: rain. 6. Boisterous : clear and fine. 7. Clear : heavy shower at noon: clear and fine. 8, 9. Fine. 10. Slight haze: fine. 11. Very fine : rain. 12. Heavy rain. 13. Showery. 14. Rain: heavy squall : clear and fine at night. 15. Cloudy. 16. Very fine. 17. Densely overcast. 18, 19. Very fine. 20. Overcast: fine. 21—25. Cloudy and fine. 26. Cloudy. 27. Cloudy and dry. 28. Clear, with hot sun. 29. Cloudless and hot. 30. Fine. 31. Hot and dry.—Mean temperature of the month 2°·57 below the average.

Boston.— Aug. 1. Cloudy: rain early A.M. : rain A.M. 2. Fine. 3. Rain : rain early A.M. : rain with thunder and lightning A.M. : rain P.M. 4. Cloudy. 5. Fine : rain at night. 6. Cloudy. 7. Stormy : rain early A.M. : rain with thunder and lightning A.M. 8. Windy: rain A.M. 9. Windy. 10. Fine. 11. Fine : rain P.M. 12. Rain. 13. Fine. 14. Rain : rain early A.M. : rain A.M. and P.M. 15. Cloudy: rain early A.M. 16. Fine : rain P.M. 17. Cloudy. 18—21. Fine. 22—24. Cloudy. 25. Fine. 26. Cloudy. 27—31. Fine.

Sandwick Manse, Orkney.—Aug. 1. Cloudy : rain. 2. Bright: clear. 3. Showers: drizzle. 4. Damp: drizzle. 5. Cloudy. 6. Cloudy: rain. 7. Showers : cloudy. 8. Cloudy : showers. 9. Showers. 10—13. Bright : clear. 14. Bright : clear: fine. 15. Clear: fine. 16. Bright: cloudy: fine. 17. Showers: rain. 18. Bright: cloudy. 19. Bright: damp. 20. Showers. 21. Rain : drizzle. 22. Rain: damp. 23. Damp. 24. Damp: cloudy. 25. Bright: cloudy. 26. Drizzle : drops. 27. Drizzle: showers. 28. Bright : clear. 29. Clear. 30. Clear: warm. 31. Clear : cloudy : warm.

Applegarth Manse, Dumfries-shire.—Aug. 1—3. Showers. 4. Fair. 5. Fine. 6, 7. Heavy rain. 8. Rain A.M. : fair P.M. 9. A shower. 10. Fair A.M. : few drops P.M. 11. Fair A.M. : rain P.M. 12. Fair. 13. Showery. 14. Showery : thunder. 15. Fair. 16. Rain P.M. 17. Heavy showers. 18. Fair and clear. 19. Heavy showers. 20. Fair. 21. Fair : a few drops. 22. Rain. 23. Heavy shower P.M. 24. One shower. 25—31. Fair and fine.

Meteorological Observations made by Mr. Thompson at the Garden of the Horticultural Society at CHISWICK, *near London; by Mr. Veall, at* BOSTON; *by the Rev. W. Dunbar, at Applegarth Manse,* DUMFRIES-SHIRE; *and by the Rev. C. Clouston, at Sandwick Manse,* ORKNEY.

Days of Month 1844. Aug.	Barometer							Thermometer							Wind				Rain			
	Chiswick Max.	Chiswick Min.	Boston 8¾ a.m.	Dumfries-shire 9 a.m.	Dumfries-shire 9 p.m.	Orkney Sandwick 9½ a.m.	Orkney Sandwick 8½ p.m.	Chiswick Max.	Chiswick Min.	Boston 8¾	Dumfries-shire Max.	Dumfries-shire Min.	Orkney Sandwick 9½ a.m.	Orkney Sandwick 8½ p.m.	Chiswick 1 p.m.	Boston	Dumfries-shire	Orkney Sandwick	Chiswick	Boston	Dumfries-shire	Orkney Sandwick
1.	29·807	29·732	29·15	29·42	29·55	29·51	29·50	71	45	57	59½	51	57	53	w.	w.	ssw.	w.	·02	·21	…	·48
2.	29·868	29·774	29·29	29·47	29·68	29·66	29·71	72	49	60	61	46½	56	54	w.	w.	sw.	w.	·11	·04	…	·22
3.	29·469	29·427	29·03	29·40	29·34	29·58	29·56	69	52	57	55	40	54½	53½	sw.	s.	e.	ne.	·15	·10	…	·04
4.	29·833	29·683	29·05	29·49	29·61	29·58	29·71	77	44	58	62	50	56½	52½	w.	nw.	w.	ne.	·01	·41	…	…
5.	29·833	29·632	29·19	29·70	29·63	29·79	29·79	72	57	64	65½	52	59	53	s.	calm	e.	e.	·37	…	…	·07
6.	29·587	29·522	28·81	29·24	29·00	29·66	29·44	71	52	66	65	53	57	54	sw.	sw.	se.	e.	·02	·37	0·75	…
7.	29·656	29·621	29·00	29·15	29·25	29·14	29·10	71	50	62	56½	50½	55	54½	sw.	w.	w.	e.	·12	·03	…	·45
8.	29·712	29·614	29·03	29·23	29·38	29·13	29·33	72	47	61·5	60	51	59	54	sw.	w.	w.	nnw.	…	·02	…	·17
9.	29·756	29·733	29·16	29·53	29·54	29·46	29·52	74	45	62	57½	51	49½	52½	w.	calm	wsw.	nw.	…	·04	…	·14
10.	29·591	29·591	29·14	29·53	29·59	29·60	29·66	76	41	61	65	54	51	51	n.	calm	w to n	nw.	…	…	0·30	·10
11.	29·783	29·550	29·27	29·60	29·50	29·65	29·67	73	54	61·5	59½	39½	53½	49	sw.	calm	s.	se.	·12	…	…	…
12.	29·608	29·374	28·80	29·43	29·53	29·67	29·66	70	54	60	63½	52	55½	51½	w.	calm	ne.	e.	·33	·40	…	…
13.	29·641	29·531	29·07	29·45	29·35	29·56	29·48	65	51	57	64	49	58	51	sw.	calm	sw.	e.	·24	…	0·30	·30
14.	29·424	29·331	28·80	29·18	29·35	29·43	29·56	65	49	57	62½	49	59	51	w.	nw.	ne.	…	·30	·25	…	…
15.	29·744	29·552	29·04	29·52	29·68	29·68	29·75	68	47	60	63	49	56	55	sw.	w.	nw.	n.	·03	·49	…	…
16.	29·919	29·836	29·31	29·75	29·53	29·77	29·71	77	57	61	58	44	56	53½	nw.	w.	w.	…	·01	…	…	…
17.	29·880	29·768	29·11	29·50	29·74	29·63	29·74	69	47	59·5	59½	50	52	50	w.	nw.	nw.	nw.	…	·21	…	…
18.	30·092	30·060	29·56	29·92	30·04	29·97	30·08	69	43	58	63½	47½	49½	52	nw.	nw.	nw.	nw.	…	…	…	…
19.	30·119	30·041	29·52	30·02	29·82	29·98	29·78	72	60	67	60	48	54	53	w.	w.	sw.	se.	·01	…	…	·30
20.	29·901	29·768	29·20	29·72	29·63	29·60	29·63	78	51	58	57	50	50	49	w.	calm	nw.	nnw.	…	…	…	·09
21.	29·747	29·727	29·10	29·58	29·58	29·59	29·60	65	52	57	57½	50	51	49	w.	calm	nw.	n.	…	…	…	·42
22.	29·727	29·698	29·12	29·53	29·52	29·57	29·55	67	42	58	58	49	51	52	w.	calm	w.	…	…	…	0·95	·30
23.	29·726	29·673	29·16	29·49	29·57	29·59	29·67	69	38	58·5	64	45	55	51½	sw.	calm	nw.	se.	…	…	…	·04
24.	29·746	29·659	29·13	29·65	29·70	29·76	29·80	71	49	57	60	47	56	52	sw.	w.	w.	w.	…	…	…	·04
25.	29·976	29·902	29·25	29·79	29·84	29·81	29·85	73	50	58	61	41	54	50½	nw.	nw.	nw.	wnw.	…	…	…	…
26.	30·052	30·007	29·39	29·84	29·92	29·85	29·88	66	47	56	60	60	54	48	nw.	nw.	nw.	wnw.	…	…	…	…
27.	30·082	30·078	29·50	29·98	30·00	29·94	30·03	71	38	53	65	41	51	49½	w.	w.	sw.	nw.	…	…	…	…
28.	30·137	30·110	29·59	30·05	30·01	30·05	30·08	78	40	60	68	43	51	53	w.	calm	sw.	w.	…	…	…	…
29.	30·138	30·113	29·58	30·05	30·01	30·08	30·03	79	42	61	68½	47	57½	54	e.	calm	w.	…	…	…	0·18	…
30.	30·182	30·127	29·55	30·04	30·07	30·03	30·09	72	42	61	68	46½	59	56	e.	calm	sw.	w.	…	…	…	…
31.	30·292	30·278	29·71	30·20	30·21	30·22	30·22	80	44	61·5	70	48	57½	54	se.	calm	sw.	se.	…	…	…	…
Mean.	29·844	29·758	29·21	29·627	29·636	29·146	29·199	71·67	47·71	59·6	61·5	47·2	54·87	52·16					1·84	2·57	2·18	2·86

LI. *Examination of a yellow substance from India called Purree, from which the pigment called Indian Yellow is manufactured. By* JOHN STENHOUSE, *Ph.D.*[*]

PURREE, the substance from which the well-known pig-ment, Indian yellow, is made, is imported into Great Britain in considerable quantity from various parts of India and China. As usually met with in commerce, purree con-sists of balls of from three to four ounces in weight, which are dark brown externally, but when broken up their interior has a deep orange colour. When viewed under the microscope, they evidently consist of small needle-shaped crystals. Purree has a very peculiar smell, very closely resembling that of castor. This circumstance has induced the belief that it is an animal matter, and it is generally regarded, even by those best acquainted with the East, as consisting of bezoars from the gall-bladders of different animals, such as the Camel, the Ele-phant, the Buffalo, &c. Another prevalent opinion is, that it is a deposit from the urine of some of these animals. From reasons which will presently appear, I am induced to believe that purree has a vegetable and not an animal origin. My attention was first directed to it by Dr. Pereira, and through the kindness of Messrs. Warington and W. De la Rue I was enabled to obtain a quantity of it for examination.

Purree is not very soluble either in water or in alcohol; its aqueous solution has a pale yellow colour, and is quite neu-tral. Æther also dissolves a little of it, and on evaporating the solution a bright yellow crystalline matter is deposited, which is slightly acid. Caustic alkalies dissolve a portion of it; their solutions have a fine rich yellow colour, but even when boiled with it not a trace of ammonia is evolved. When purree is burned it leaves a considerable quantity of ashes,

[*] Communicated by the Author.

which chiefly consist of magnesia. The ashes also contain some potash in the state of carbonate, and a little lime, but no phosphoric acid. Purree dissolves very readily in most acids. The one which I usually employed was the acetic, in which, when assisted by heat, it is exceedingly soluble. The colour of the solution is a deep reddish-brown; on cooling it deposits a quantity of dark brown flocks, and on standing for a few days a quantity of deep yellow crystals arranged in small stars appear in the liquid, the taste of which is exceedingly nauseous. The most convenient way of obtaining these crystals in a pure state is the following:—

Dissolve the purree, cut into small pieces, in boiling water containing a considerable quantity of acetic acid, and filter the liquid to remove the insoluble matter, which consists chiefly of dark brown flocks and of some other impurities. The clear liquid is to be treated with a solution of acetate of lead so long as it occasions a precipitate, which is very bulky and has a brownish-yellow colour. The precipitate is to be collected and washed with cold water, and to be decomposed by sulphuretted hydrogen; it is then to be repeatedly boiled with spirits of wine, in which the yellow crystals are pretty soluble. On the cooling of the liquid the crystals are deposited in great quantity; they are long flat needles arranged in stars, and have a deep yellow colour and considerable lustre. They redden litmus pretty strongly, and consist of a vegetable acid united in the crude substance (purree) with magnesia. They are still far from pure, and retain a considerable quantity of magnesia, from which it is extremely difficult to free them. The crystals should then be dissolved in a hot solution of carbonate of soda and filtered. When the solution is neutralized with muriatic acid, they are again deposited on the cooling of the liquid. The crystals should be collected, washed with cold water, in which they are very slightly soluble, to remove adhering muriatic acid, and dried by pressure. They should again be dissolved in hot water and be precipitated by acetate of lead. The lead salt should then be decomposed by sulphuretted hydrogen, and the crystals dissolved out by hot spirits of wine. The crystals have now only a faint shade of yellow, and are to be still further purified by being five or six times crystallized from spirits of wine. They are then almost entirely pure, though on being burned on platinum foil they still leave a mere trace of magnesia, so small as to be scarcely perceptible. I was not able to remove this minute portion of magnesia, even by repeatedly crystallizing the acid from æther.

I. 0·3975 grm. substance dried at 212° F. and burned with chromate of lead, gave 0·7979 carbonic acid and 0·1583 water.

II. 0·4294 gave 0·866 carbonic acid and 0·1706 water.
III. 0·437 gave 0·882 carbonic acid and 0·175 water.

				Calculated numbers.	
I.	II.	III.		Atom.	Per cent.
C 55·20	55·002	55·04	Carbon	20 = 1500	55·30
H 4·42	4·414	4·45	Hydrogen	9 = 112	4·20
O 40·38	40·584	40·51	Oxygen	11 = 1100	40·50
100·00	100·000	100·00		2712	100·00

These analyses appear therefore to give $C_{20} H_9 O_{11}$ as
the formula of the acid to which I shall give the provisional
name of the purreic. It is but little soluble in cold water, but
dissolves pretty readily in boiling water, from which it is de-
posited in long slender silky needles of a faint yellow colour.
The alkalies, and especially ammonia, immediately change its
solution to a fine deep yellow colour. The acid is exceed-
ingly soluble in hot spirits of wine, which is its best solvent.
When water is added to the solution, the greater portion of
the acid is immediately deposited in small crystals. It also
dissolves to a considerable extent in æther, and is deposited in
needles arranged in stars on the evaporation of the liquid:
its taste is at first sweetish and then slightly bitter. In ap-
pearance it has a considerable resemblance to berberine, but
has rather a lighter colour. The quantity of acid in the crude
purree is exceedingly great, amounting, I should think, to
nearly half its weight. Purreic acid was also carefully exa-
mined for nitrogen, but not a trace of it could be detected.
The acid does not precipitate the salts of silver, lime, barytes,
strontian, or magnesia, but it gives a deep yellow precipitate
with acetate of lead. Its combinations with the alkalies are
very soluble and slightly crystalline. The saturating power
of the acid appears to be very feeble, as a few drops of an
alkali added to a strong solution of the acid give it an alka-
line reaction. When purreic acid is neutralized with an
alkali, it gives bright yellow precipitates with solutions of most
of the bases, as silver, lime, barytes, magnesia, &c. With
protosulphate of iron it gives a deep green precipitate; these
precipitates are all very flocculent and pretty soluble, so that
it is difficult to obtain them of an uniform nature. The salt
which I employed to determine the atomic weight was the
lead compound, and even with this I found considerable diffi-
culty in obtaining satisfactory results. The lead salt was
prepared by adding an alcoholic solution of acetate of lead to
a hot solution of the acid, also in alcohol. The salt falls as
an orange yellow, somewhat gelatinous precipitate. It was
boiled repeatedly with spirits of wine, in which it is nearly in-

soluble, and washed with hot alcohol till all excess of acetate of lead was removed. When highly heated the salt melts and gives off vapours, which condense in long bright yellow crystals on the edge of the crucible. These crystals constitute a substance which will presently be described.

I. 0·4835 salt, dried at 212° F., gave 0·128 oxide and 0·036 lead = 34·34 per cent. of oxide.

Per cent.

II. 0·4395 gave 0·125 oxide and 0·0235 lead = 34·21

III. 0·522 gave 0·152 oxide and 0·0240 lead = 34·00

IV. 0·4395 gave 0·125 oxide and 0·0235 lead = 34·09

Mean...... 34·16

These determinations were made with three different quantities of the lead salt.

I. 0·5332 substance burned with chromate of lead gave 0·700 carbonic acid and 0·142 water.

II. 0·541 substance gave 0·716 carbonic acid and 0·15 water.

Calculated numbers.

	I.	II.		Atom.		Per cent.
C	35·86	36·11	Carbon	. 20 = 1500	36·5
H	2·95	3·08	Hydrogen	. 9 = 112	2·7
O	27·03	26·65	Oxygen	. 11 = 1100	26·8
PbO	34·16	34·16	Oxide of lead	1 = 1394	34·0
	100·00	100·00			4106	100·0

From these analyses it is evident that the oxide of lead has united with the acid without removing an atom of water, as is usually the case, unless with very feeble acids. The calculated atomic weight of the acid from this salt is 2712, and the number found is 2662. Purreic acid forms only one salt with lead, as the same compound was also obtained when subacetate of lead was employed.

When purreic acid is heated considerably above 212° F. it melts, and if the heat is still further increased it begins to sublime. The best way of obtaining this sublimate in bright yellow crystals of from 1 to 1½ inch long, is cautiously to heat pretty strongly a quantity of the impure acid in Mohr's subliming apparatus. A very large proportion of the acid is charred, but a quantity of fine large crystals may be readily procured, attached for the most part to the under side of the diaphragm. These crystals are not purreic acid, but a neutral body which does not redden litmus. It is but slightly soluble in either acid or alkaline solutions, or in water, alcohol or æther. Its alcoholic solution does not precipitate neutral acetate of lead, nitrate of silver, or the chlorides of lime or barytes, but with basic acetate of lead it gives a yellow mucilaginous precipitate.

I. 0·4194 substance dried at 212° F. and burned with oxide of copper, gave 1·045 carbonic acid and 0·1357 water.

II. 0·375 gave 0·9339 carbonic acid and 0·1222 water.

III. 0·318 burned with chromate of lead gave 0·796 carbonic acid and 0·107 water.

	I.	II.	III.		Atom.	Per cent.
C	67·95	67·92	68·20	Carbon	13 = 975	68·42
H	3·59	3·62	3·73	Hydrogen	4 = 50	3·62
O	28·46	28·46	28·07	Oxygen	4 = 400	27·96
	100·00	100·00	100·00		1425	100·00

These numbers correspond pretty closely with the formula $C_{13} H_4 O_4$. The substance is pretty difficult to burn, which accounts for the deficiency of carbon in the first two analyses, when oxide of copper was employed. I propose the provisional name purrenone for this body, which in the mode of its formation somewhat resembles alizarine, both being neutral bodies, obtained by subliming feebly acid colouring matters. Purrenone may also be procured by subliming the lead salt. When burned on platinum foil it leaves no residue.

When purreic acid is treated in the cold with nitric acid of ordinary strength, it dissolves apparently without decomposition. With the assistance of heat it is rapidly decomposed with the abundant evolution of nitrous gas. When the nitric acid is driven off by gently evaporating to dryness on the water-bath, a crystalline acid is obtained, which is much more soluble than the purreic acid, and stains the skin or paper of a deep yellow colour. When its concentrated solution is neutralized by potash, a reddish-yellow salt precipitates, crystallized in pretty large needles. When heated it detonates, and thus appears to contain nitrogen. From want of material I was unable to continue its investigation, but I may probably resume it at a future time.

When purreic acid is boiled with peroxide of manganese no action takes place, but on the addition of sulphuric acid it is decomposed with the production of formic acid. When purreic acid is strongly heated with solid potash, it dissolves with a bright scarlet colour; on neutralizing with an acid the colour disappears and the purreic acid is precipitated, converted apparently into a resinous matter.

I would conclude for the present by observing, that notwithstanding all the reports to the contrary, it appears highly probable that the purree of commerce is the juice of some tree or plant, which, after it has been expressed, has been saturated with magnesia and boiled down to its present consistence.

Glasgow, September 19, 1844.

LII. *On the Propagation of Waves in a Resisted Fluid; with a new Explanation of the Dispersion and Absorption of Light, and other Optical Phænomena. By the* Rev. M. O'Brien, *Professor of Natural Philosophy and Astronomy in King's College, London, and late Fellow of Caius College, Cambridge**.

1. TWO different hypotheses may be made respecting the mode of action of the particles of a transparent substance upon the vibrations of the æthereal fluid within it. The first is this: that the transparent substance exerts upon each element of the æthereal fluid forces which depend simply upon the displacements of that element relatively to the contiguous particles of matter. This will be the case when the *amplitudes* of the æthereal vibrations (*i. e.* the maximum excursions of the elements from their positions of equilibrium) are extremely small compared with the intervals between the particles of the transparent substance, but not otherwise. The second hypothesis is this: that the forces exerted by the transparent substance upon any element of the æthereal fluid are of the same nature as the resistances experienced by a set of particles moving through a resisting medium, depending, not upon the displacements of the element relatively to the contiguous particles of matter, but upon the velocity, or rather the state of motion of the element. This will be the case when the amplitudes of the æthereal vibrations are large compared with the intervals between the particles of the transparent substance.

2. The equations employed by M. Cauchy, in investigating the vibratory motion of two mutually penetrating systems of particles, are obtained by assuming that the *relative* motion of any two contiguous particles is extremely small compared with the distance between them. Now, as we must suppose that the displacements of the particles of a transparent substance caused by the vibrations of the æther are always extremely small compared with the amplitudes of the æthereal vibrations †, the motion of any æthereal particle *relatively* to a contiguous material particle will be very nearly the same thing as the *actual* motion of the former particle in space. Hence, in order that M. Cauchy's assumption may be true, the displacements of each particle of æther from its equilibrium position must be extremely small compared with its distance from any contiguous particle of matter; which cannot be the case unless the amplitudes of the æthereal vibrations be very small compared

*. Communicated by the Author.

† The truth of this statement is shown by a result obtained from M. Cauchy's equations, namely the relation between $\alpha_{,}$ and α at the bottom of page 214 of the 20th volume of the Philosophical Magazine, S. 3.

with the intervals between the material particles. Hence it appears that M. Cauchy's equations are obtained, *not only* upon the supposition that the relative motion of any two contiguous particles of æther is extremely small compared with the distance between them, *but also* upon the assumption that the amplitudes of the æthereal vibrations are extremely small compared with the intervals between the particles of transparent substances.

When we consider that, in all probability, the intervals between the particles of matter are extremely minute compared even with the length of a luminous wave, and that the amplitude of a wave is not necessarily, nor even generally, extremely small compared with its length, we are certainly not warranted in assuming that the amplitudes of the æthereal vibrations are less than the intervals between the particles of matter; and I think therefore that the correctness of the first of the above hypotheses may be fairly questioned on this ground, except in the case of light of the weakest intensity.

3. An explanation of the dispersion and absorption of light has been deduced from M. Cauchy's equations* in the particular case where the number of æthereal particles in any space is the same as that of the material. The explanation of dispersion appears to be satisfactory enough, but that of absorption is really fallacious for two reasons; namely, first, because it is founded upon a perfectly gratuitous rejection of the positive index in the expression for the disturbance, which is of the form

$$a\, e^{\pm hz} \cos(n\,t - k\,z)$$

(see a paper in the Cambridge Transactions, vol. viii. p. 3); and, secondly, because I think it has been proved, in the paper just cited, that, in the case of light passing from vacuum into a transparent substance, the equations of connection at the surface of separation cannot be satisfied if we reject either the positive or the negative index in the above expression, but that the integral representing the disturbance within the transparent substance must necessarily be of the form

$$a\, \{e^{hz} \cos(n\,t - k\,z + \omega) + e^{-hz} \cos(n\,t - k\,z - \omega)\},$$

an integral which indicates the very reverse of absorption.

4. M. Cauchy states that his equations are capable of accounting, not only for dispersion and absorption, but also for several of the remarkable phænomena exhibited by light when it is transmitted through certain crystals and liquids; but

* I may be allowed to mention here, that when I wrote a paper on this subject in 1842, (Phil. Mag. S. 3. vol. xx. p. 201) I was not aware that Prof. Lloyd had read one previously on Dispersion and Absorption before the Royal Irish Academy.

Prof. MacCullagh has clearly shown that this statement must be received with considerable limitation.   On the whole I think we have good reason to doubt the correctness of the *first* of the above *hypotheses*, both on the *à priori* ground above stated, and because it appears to be capable of accounting for little more than the dispersion of light.

5. The *second hypotheses* has not yet been taken up by any writer upon physical optics, so far as I am aware, but it is decidedly worthy of attention and leads to several important results.   The object of the present communication is simply to make a beginning in the application of this hypothesis to the explanation of optical phænomena, and to show that it may possibly prove of much service in advancing the undulatory theory of light.

Let us suppose then that the amplitudes of the æthereal vibrations are large compared with the intervals between the particles of transparent substances, and that consequently the motion of each element of the æthereal fluid is resisted by forces (the reactions of the material particles) which have this peculiarity, that they depend simply upon the *state of motion* of the element, but not upon its *displacements* from its equilibrium position.

6. Before we proceed, however, it will be necessary to make a few general remarks upon the nature of resistances, reserving mathematical details for a future communication.

It is commonly assumed that, when a spherical particle moves in a resisting medium, the force of resistance, at any time, is a function simply of the velocity at that time, and acts in a direction opposite to that of the motion.   This must undoubtedly be true when the motion is uniform and rectilineal, and very nearly true when the curvature of the path of the particle is not great, and the variations of the velocity are small compared with the velocity itself.   But in general the magnitude and direction of the resistance at any instant must depend upon the magnitude and direction, not only of the velocity *at* that instant, but also of the velocities with which the particle was moving during a certain interval *preceding* that instant.   In other words, the magnitude and direction of the resistance at any time must in general depend, not only upon the velocity, but also upon the differential coefficients of the velocity with respect to the time, and upon the curvature of the path of the particle.   The truth of this statement is manifest from the fact, that the impulse given to the medium by the particle at any instant produces more or less motion in the medium, which must in some degree affect the subsequent motion of a particle.   We may not therefore assume that the

medium produces no resistance along the normal of the path of the particle: in fact, in the case of a particle describing a very small oval with a small velocity, the principal resistance, it seems to me, would be along the normal.

7. From the consideration of a particle moving in a resisting medium, we may easily pass to the consideration of the resistance experienced by a moving fluid in which a great number of fixed particles are immersed at regular intervals from each other. In such a case we must suppose that the magnitude and direction of the resistance on any element of the fluid depends upon the velocity of that element, the differential coefficients of the velocity with respect to the time, and the curvature of the path described by the element. We must also, I think, suppose that in general the resistance on any element depends in some degree upon the state of motion of the elements which surround it; but, in the particular case where the state of motion varies very slowly in passing from element to element (which will be the only case we shall have to consider), it will not be necessary to take this into account. There will also be a certain amount of internal agitation produced in each element, which will depend upon the magnitude of the resisting particles and the distances between them. When the particles are placed at very small intervals from each other, and their magnitudes are very small compared with these intervals, this internal agitation will be inconsiderable compared with the motion of translation of the element. We suppose, of course, that each element of the fluid (*i. e.* each portion of it throughout which the state of motion may be regarded as sensibly the same) includes a great number of the resisting particles within it, which amounts to supposing that the variation of the state of motion is sensible only when we pass over a distance considerably greater than the intervals between the resisting particles; or, in other words, that the length of the wave (supposing the motion of the medium to be undulatory) is extremely large compared with the intervals between the resisting particles.

8. We shall now apply these considerations to the case of plane transversal waves transmitted through the æther, supposed to be resisted by the particles of an uncrystallized body.

Let us choose three rectangular axes, and assume the axis of z so as to coincide with the direction of propagation. Conceive the æthereal medium to be divided into a series of elementary slices by planes parallel to that of xy, the thickness of each slice being very small compared with the length of the wave, but very much larger than the intervals between the particles of matter, which we shall suppose to be so many phy-

sical points placed at equal distances from each other. Then
each of the slices will oscillate parallel to the plane of xy, the
velocity being the same at every point of the same slice, and,
according to what has been above stated, each slice will expe-
rience a resistance to its motion depending in magnitude and
direction upon the velocity of the slice, the differential coeffi-
cients of the velocity with respect to the time, and the curva-
ture of the path described by any point of the slice (observing
that every point of the slice describes the same path, or, in
other words, that the motion of the slice is simply one of trans-
lation). On account of the extreme smallness of the intervals
between the resisting particles compared with the length of the
wave, the internal agitation of the æther composing any slice
will be inconsiderable compared with the general motion of
translation of the slice; something like the ripple on the sur-
face of a great wave of water.

The forces which act upon any slice will arise from three
causes:—1st, the unequal displacements of the slices from their
equilibrium position parallel to the plane of xy; 2nd, the ge-
neral resistance to the motion of translation of the slice caused
by the resisting particles; 3rd, the disarrangement of the æther
due to the internal agitation. The forces arising from the
third cause we shall neglect, on account of the smallness of
the internal agitation, and because these forces will be of the
nature of internal forces, and therefore produce little or no
effect on the motion of translation of any slice.

9. Let z be the distance of any particular slice from the
plane of xy, and $\xi\,\eta$ its *displacements* parallel to the axes of x
and y. Then the accelerating forces parallel to the axes of x
and y due to the first of the causes above mentioned will be
(as is well known)

$$A\,\frac{d^2\xi}{dz^2}, \quad A\,\frac{d^2\eta}{dz^2},$$

A being a constant depending upon what may be called the
lateral or *transversal* elasticity of the æther.

Let the unknown resistance on the slice be resolved into
two forces T and N, the former acting parallel to the tangent
of the curve which any point of the slice describes, tending to
check the tangential motion of the slice, as we may call it; and
the latter acting parallel to the normal tending to check the
motion of deflexion towards the centre of curvature. Then,
observing that $\dfrac{1}{v}\dfrac{d\xi}{dt}$ and $\dfrac{1}{v}\dfrac{d\eta}{dt}$ $\left(\text{where } v^2 = \left(\dfrac{d\xi}{dt}\right)^2 + \left(\dfrac{d\eta}{dt}\right)^2\right)$
are the cosine and sine of the angle which the tangent makes
with the axis of x, we have the following equations of motion:

$$\frac{d^2 \xi}{d t^2} = A \frac{d^2 \xi}{d z^2} - \frac{1}{v}\left(T \frac{d \xi}{d t} - N \frac{d \eta}{d t}\right). \quad \text{. . . . (1.)}$$

$$\frac{d^2 \eta}{d t^2} = A \frac{d^2 \eta}{d z^2} - \frac{1}{v}\left(T \frac{d \eta}{d t} + N \frac{d \xi}{d t}\right). \quad \text{. . . . (2.)}$$

Multiplying (2.) by $\sqrt{-1}$ and adding it to (1.), we find

$$\left.\begin{array}{l}\dfrac{d^2 (\xi + \eta \sqrt{-1})}{d t^2} + \dfrac{1}{v}\, (T + N \sqrt{-1})\, \dfrac{d (\xi + \eta \sqrt{-1})}{d t} \\[2mm] \quad = A \dfrac{d^2 (\xi + \eta \sqrt{-1})}{d z^2}\end{array}\right\} \quad \text{. . (3.)}$$

10. To obtain a solution of this equation, we shall suppose that the light is circularly polarized, *i. e.* that each slice has a circular motion of translation parallel to the plane of $x\,y$. In this case N T and v (which in general are functions of t and z) will manifestly cease to vary with t; and their variation with z also will be insensible compared with that of ξ and η; for ξ and η go through all their values when z increases by a wave's length, whereas the radius of the circle of vibration, and therefore N T and v, can suffer only an extremely small change for so minute an increment of z, except the light be so rapidly absorbed that the substance may be regarded as opake, and not transparent, as we have supposed. Hence we may without sensible error integrate the equation (3.) on the supposition that N T and v are independent of z as well as of t.

Now, the vibrations being circular, the expressions for ξ and η must be of the form

$$\xi = u \cos (n\,t - k\,z), \qquad \eta = u \sin (n\,t - k\,z),$$

where n and k are constants and u is independent of t. (We suppose n and k here to be essentially positive, which amounts to supposing that the wave is propagated in the *positive* direction along the axis of z.)

Hence we have

$$\xi + \eta \sqrt{-1} = u\, e^{(nt-kz)\sqrt{-1}};$$

and if we substitute this in equation (3.), we find

$$\left.\begin{array}{l}\left\{-n^2 + \dfrac{n}{v}\, (T \sqrt{-1} - N)\right\} u \\[2mm] \quad = A \left\{\dfrac{d^2 u}{d z^2} - 2\,k \dfrac{d u}{d z} \sqrt{-1} - k^2 u\right\}\end{array}\right\} \quad \text{. . (4.)}$$

To solve this linear equation put, as usual, $u = a\, e^{-hz}$, and we find

$$-n^2 + \frac{n}{v}\, (T \sqrt{-1} - N) = A\, (h^2 + 2\,k\,h \sqrt{-1} - k^2),$$

which, equating possible and impossible parts, gives

$$\mathrm{A}\,(k^2 - h^2) = n^2 + \frac{\mathrm{N}}{v}\,n, \quad . \quad . \quad . \quad . \quad . \quad (5.)$$

$$2\,\mathrm{A}\,k\,h = \frac{\mathrm{T}}{v}\,n; \quad . \quad . \quad . \quad . \quad . \quad . \quad (6.)$$

and hence we find

$$2\,\mathrm{A}\,k^2 = n\left\{ \sqrt{\left(n + \frac{\mathrm{N}}{v}\right)^2 + \left(\frac{\mathrm{T}}{v}\right)^2} + \left(n + \frac{\mathrm{N}}{v}\right) \right\}, \quad (7.)$$

$$2\,\mathrm{A}\,h^2 = n\left\{ \sqrt{\left(n + \frac{\mathrm{N}}{v}\right)^2 + \left(\frac{\mathrm{T}}{v}\right)^2} - \left(n + \frac{\mathrm{N}}{v}\right) \right\}. \quad (8.)$$

We reject the negative sign of the radical in these expressions because it would make k impossible; also, since A, k, T and v are essentially positive, equation (6.) shows that h must be positive also. It appears therefore that the equation (4.) admits only of one solution, namely $a\,e^{-hz}$, where h is a positive quantity given by equation (8.) and a an arbitrary constant. The results we have thus obtained may be found immediately by putting $\xi + \eta \sqrt{-1} = a\,e^{(nt-kz)\sqrt{-1}-hz}$ in the equation (3.).

11. We have assumed that u (the radius of the circle of vibration) suffers no sensible change when z increases by a wave's length (λ suppose); hence the ratio $a\,e^{-hz} : a\,e^{-h(z+\lambda)}$ must be very nearly unity, and therefore $h\lambda$ must be a very small quantity: now $\lambda = \frac{2\pi}{k}$, therefore $\frac{h}{k}$ must be a very small ratio, and, it is manifest from the equations (7.) and (8.), that this will be the case when $\frac{\mathrm{T}}{v}$ is very small compared with n or with $n + \frac{\mathrm{N}}{v}$. Now n, as is well known, is an extremely large number, therefore all that is necessary to the correctness of the above integration is this, that the tangential resistance (T) be not very large compared with the velocity of vibration (v).

12. Since h is essentially positive, it follows that u continually diminishes as the wave advances, and it appears from the equation $u = a\,e^{-hz}$ that the rate of decrease of u is $h\,u$; h is therefore the *proportional* rate of decrease of the amplitude of vibration.

13. We have thus obtained the laws of propagation of circularly polarized light in uncrystallized substances, which may be stated thus:

The time of vibration being $\frac{2\pi}{n}$, the velocity of propagation

will be $\frac{n}{k}$, where k is given by the equation

$$2 \, \mathrm{A} \, k^2 = n \left\{ \sqrt{\left(n + \frac{\mathrm{N}}{v}\right)^2 + \left(\frac{\mathrm{T}}{v}\right)^2} + \left(n + \frac{\mathrm{N}}{v}\right) \right\}.$$

Also the amplitude of vibration will decrease as the wave advances, the proportional rate of decrease being h, where h is given by the equation

$$2 \, \mathrm{A} \, h^2 = n \left\{ \sqrt{\left(n + \frac{\mathrm{N}}{v}\right)^2 + \left(\frac{\mathrm{T}}{v}\right)^2} - \left(n + \frac{\mathrm{N}}{v}\right) \right\}.$$

The quantities $\frac{\mathrm{N}}{v}$ and $\frac{\mathrm{T}}{v}$ are unknown, but they can vary only with n and the amplitude of vibration. Experiment shows that in ordinary cases, where the light is of moderate intensity, the velocity of propagation does not sensibly depend upon the intensity[*], and therefore we may conclude, from the above expression for k, that $\frac{\mathrm{N}}{v}$ and $\frac{\mathrm{T}}{v}$ do not sensibly depend upon the amplitude, and consequently vary only with n, when the light is not intense.

14. The application of these results to the general explanation of the dispersion and absorption of light is obvious; they show that the velocity of propagation depends upon the colour, that light is weakened or absorbed in passing through transparent substances, and that the degree of absorption is different for different colours.

15. It is worth remarking, that the absorption is produced by the tangential resistance (T), for if we put $\mathrm{T} = 0$ we find $h = 0$. If, on the other hand, we suppose the normal resistance (N) to be zero, the above formulæ for h and k are not materially altered; the existence of a normal resistance is not therefore in any way necessary to the explanation here given of dispersion and absorption.

16. So far we have assumed nothing respecting the resistances exercised by the particles of matter upon the æthereal fluid beyond the existence of such forces; we shall now examine what consequences result from the supposition that the resistances do not sensibly interfere with the principle of the superposition of small motions.

The first consequence that obviously presents itself is this,

---

[*] The difficulty of observing the exact positions of the fringes of diffraction formed by strong light of varying intensity, seems to show that the velocity of propagation may depend in some degree upon the intensity.

that the laws of propagation just obtained for circular vibrations are equally true for elliptical and rectilineal vibrations; for, as is well known, we may produce elliptically or plane polarized light by superposing two rays of circularly polarized light.

The second consequence is this, that

$$\frac{T}{v} = C_1 - C_3 n^2 + C_5 n^4 \ldots\ldots \text{\&c.}$$

$$\frac{N}{v} = n (C_2 - C_4 n^2 + C_6 n^4 \ldots\ldots \text{\&c.}),$$

where $C_1 C_2 C_3 \ldots\ldots$ &c. are constants independent of n and of the amplitude of vibration. I shall reserve the proof of this for my next communication, in which I shall also attempt an explanation of the rotation of the plane of polarization produced by certain liquids, the peculiar absorbing power of tourmaline, and some other phænomena.

Balham, Surrey, September 24, 1844.

LIII. *On Specific Heat.* By J. P. Joule, *Esq.*[*]

THE well-known law which applies to the specific heat of many simple bodies, while interesting in itself as one of the doctrines of physical science, is of great importance to theoretical chemistry as a criterion for the determination of atomic weights. Dulong and Petit, the philosophers who first announced that the specific heat of simple bodies is inversely proportional to their atomic weights, proved their proposition by experiment in the case of several solid (chiefly metallic) bodies. Subsequently several attempts have been made to discover the law of the specific heat of gases, liquids and compound bodies. With regard to gases, Haycraft[†], and subsequently De la Rive and Marcet[‡], have attempted to prove that under the same pressure and volume all gases have the same specific heat. Unfortunately, the practical difficulty of ascertaining the specific heat of aëriform fluids is so great, that considerable uncertainty exists in the results obtained by the most skilful experimenters, and hence we find that the law of Haycraft, De la Rive and Marcet has failed to be confirmed by the

researches of De la Roche and Berard, Dr. Apjohn *, and Du-
long. With compound bodies, however, experimenters have
met with better success. Neumann†, in the cases of the car-
bonates and sulphates of protoxides, has pointed out the fact
that the specific heat in each of these classes is inversely pro-
portional to the atomic weight. His researches on some
oxides and sulphurets also conducted him to a similar law for
each of these species of compounds.

Of late years no philosopher has made more numerous or
more accurate experiments on specific heat than Mr. V. Reg-
nault. The investigations of this eminent chemist were, in
the first instance, directed to the specific heat of simple solid
bodies‡: subsequently they have been directed to a great va-
riety of compound bodies§. By these researches Regnault
has given the law of Dulong and Petit all the confirmation
that could be desired, and has also proved the correctness of
Neumann's extension of that law to classes of chemical com-
pounds. He has stated a general law in the following terms:
—" In all compound bodies of the same atomic composition,
and of similar chemical constitution, the specific heats are in-
versely proportional to the atomic weights."

Regnault remarks that the above law holds good only within
certain limits, and that the slight differences which are ob-
served between the results of theory and observation are not
wholly to be attributed to mere errors of experiment. He
says that if the specific heat were taken for the temperature at
which the bodies present the greatest analogy in their chemi-
cal and physical properties, the most complete isomorphism,
the law would probably hold good rigorously.

Now, without denying altogether the influence of a change
of state on the specific heat of a body, I think it may be fairly
doubted whether it is really so great as is generally imagined.
Impressed with this idea, I have been induced to draw up a
Table, in which the theoretical specific heats of a variety of
bodies impartially selected are calculated on the hypothesis
that the capacity for heat of any simple atom remains the same
in whatever chemical combination it enters. The law implied
by this hypothesis is, that the specific heat of all bodies is di-
rectly as the number of atoms in combination, and inversely
as the equivalent.

---

* Trans. of the Royal Irish Academy, vol. xviii. part 1. p. 16. (or Phil.
Mag. S. 3. vol. xii. p. 101.)
† Poggendorff's *Annalen*, vol. xxiii.
‡ *Annales de Chimie*, 1840, vol. lxxv. p. 1.
§ Ibid., 1841, vol. i. p. 129.

Name of substance.	Formula adopted.	No. of atoms divided by equivalent.	Theoretical sp. heat.	Actual sp. heat.	Experimenter.
Water	H_2O	$\frac{8}{9}$	1000	1000	[Berard.
Hydrogen	H_2	$\frac{4}{2}$	6000	3294	De la Roche and
Oxygen	O	$\frac{1}{8}$	375	236	do.
Sulphur	S	$\frac{1}{16}$	188	188	Dulong and Petit.
Iodine	I	$\frac{2}{63}$	48	54	V. Regnault.
Carbon	C	$\frac{1}{12}$	250	241	do.
Lead	Pb	$\frac{1}{103}$	29	29	Dulong and Petit.
Zinc	Zn	$\frac{1}{33}$	91	93	do.
Copper	Cu	$\frac{1}{32}$	94	95	do.
Mercnry	Hg	$\frac{1}{101}$	30	33	do.
Protoxide of lead	PbO	$\frac{2}{111}$	54	51	V. Regnault.
Protoxide of copper ...	CuO	$\frac{2}{40}$	150	142	do.
Magnesia	MgO	$\frac{2}{21}$	286	244	do.
Peroxide of iron	Fe_2O_3	$\frac{5}{78}$	192	167	do.
Subsulphuret of copper	Cu_2S	$\frac{3}{80}$	113	121	do.
Protosulphuret of lead	PbS	$\frac{2}{119}$	50	50	do.
Protosulphuret of iron	FeS	$\frac{2}{43}$	140	136	do.
Bisulphuret of iron ...	FeS_2	$\frac{3}{59}$	153	130	do.
Chloride of sodium ...	Na_2Cl_2	$\frac{4}{58}$	207	214	do.
Protochlor. of copper .	Cu_2Cl_2	$\frac{4}{100}$	120	138	do.
Chloride of lead	$PbCl_2$	$\frac{3}{139}$	65	66	do.
Iodide of silver	Ag_2I_2	$\frac{4}{334}$	51	62	do.
Iodide of lead	PbI_2	$\frac{3}{230}$	39	43	do.
Sulph. acid, sp. gr. 1·85	SO_3H_2O	$\frac{7}{49}$	429	350	Dalton.
Carbonate of potash...	$2KO+CO_4$	$\frac{8}{139}$	194	216	V. Regnault.
Carbonate of lead......	$2PbO+CO_4$	$\frac{8}{268}$	101	86	do.
Sulphate of lead	$PbOSO_3$	$\frac{6}{152}$	118	87	do.
Sulphate of potash ...	$KOSO_3$	$\frac{6}{88}$	204	190	do.
Chlorate of potash ...	$KOCl_2O_5$	$\frac{9}{125}$	220	210	do.
Nitrate of potash	KON_2O_5	$\frac{9}{102}$	265	238	do.
Alcohol	$C_2H_{10}O+H_2O$	$\frac{16}{46}$	1043	622	Despretz.

The substances chosen in the construction of the above
Table are, as I have said, impartially selected. I have omitted
some in which the theory was found to agree perfectly with
experiment, whilst I have inserted others (as, for instance, al-
cohol) which appear to disagree with the theory very consi-
derably. On the whole the coincidence between the theore-
tical and experimental results is such that I think chemists
will agree with me in believing that the law of Dulong and

Petit, with regard to simple atoms, is capable of a greater degree of generalization than we have hitherto been inclined to admit.

LIV. *Demonstration of the proposition that every function* F (μ, ω) *which does not become infinite between the limiting values* -1 *and* 1 *of* μ *and* 0 *and* 2π *of* ω *may be expanded in a series of* Laplace's *Coefficients.*

To the Editors of the Philosophical Magazine and Journal.
GENTLEMEN,

YOU will not perhaps think the following concise demonstration of this most important proposition unworthy of your consideration, based though it be on demonstrations already before the public.   I am, your obedient Servant,
PROCUL.

Let $\mu\mu' + \sqrt{1-\mu'^2}\sqrt{1-\mu^2}\cos(\omega'-\omega) = p$;
and suppose that

$(1+c^2-2cp)^{-\frac{1}{2}} = P_0 + P_1 c + P_2 c^2 + \ldots\ldots + P_i c^i + \ldots\ldots$

then it is known* that P_0, P_1...... are some of Laplace's coefficients. Differentiate with respect to c, multiply by $2c$, and add the result to the above, and we have

$\dfrac{1-c^2}{(1+c^2-2cp)^{\frac{3}{2}}} = P_0 + 3P_1 c + \ldots\ldots + (2i+1)P_i c^{i1} + \ldots\ldots$

When $c = 1$ this series equals *zero,* except when $p = 1$, in which case it appears to be indeterminate; but it is easy to show that in that case it equals *infinity,* for each of the coefficients P_0, P_1...... becomes *unity* when $p = 1$†. It is not difficult‡, moreover, to show that when $p = 1$, $\omega = \omega'$ and $\mu = \mu'$. Hence we arrive at this remarkable result, that the series

$P_0 + 3P_1 + 5P_2 + \ldots\ldots + (2i+1)P_i + \ldots\ldots$

equals *zero* for all values of the variables, except when $\mu = \mu'$ and $\omega = \omega'$, in which particular case it equals *infinity.* The series is therefore *discontinuous,* and will destroy any finite function by which it is multiplied, except for those particular values of the variables; it will destroy the multiplying function even for values of the variables differing by the smallest infinitesimal quantities from those which satisfy the conditions $\mu = \mu'$ and $\omega = \omega'$; this is because the value of the series is discontinuous, and *per saltum* passes from zero to infinity. Having premised thus far, if we integrate the series as it stands, term after term, then by the known§ property of Laplace's co-

* Pratt's Mechanical Philosophy, Second Edition, p. 161.
† Ibid. p. 160.   ‡ Ibid. p. 166.   § Ibid. p. 165.

efficients every term but the first vanishes; and, since $P_0 = 1$, we have

$$4\pi = \int_{-1}^{1} \int_{0}^{2\pi} \{ P_0 + 3P_1 + \dots + (2i+1)P_i + \dots \} d\mu'\, d\omega'.$$

Multiply both sides by F (μ, ω) and divide by 4π, and observing that the function may be written inside the signs of integration, because μ and ω are variables independent of μ' and ω', we have

$$\mathrm{F}(\mu,\omega) = \frac{1}{4\pi} \int_{-1}^{1} \int_{0}^{2\pi} \mathrm{F}(\mu,\omega) \{ P_0 + \dots + (2i+1)P_i + \dots \} d\mu' d\omega'.$$

But, by what has been shown above, the whole quantity under the signs of integration vanishes, except when $\mu = \mu'$ and $\omega = \omega'$, and it vanishes even for values of μ' and ω', differing by the smallest infinitesimal quantities from these. Hence the equation may be written

$$\mathrm{F}(\mu,\omega) = \frac{1}{4\pi} \int_{-1}^{1} \int_{0}^{2\pi} \mathrm{F}(\mu',\omega') \{ P_0 + \dots + (2i+1)P_i + \dots \} d\mu' d\omega'.$$

But P_i, and therefore the general term of this series, since the operations performed on the terms are *linear*, is a function of μ and ω, which satisfies Laplace's equation. Hence F (μ, ω) can be expanded in a series of Laplace's coefficients.—Q.E.D.

N.B. It cannot be objected to this theorem, that because a series is made use of the value of which becomes *infinite* under certain circumstances, therefore the results are not to be depended upon. For the integration modifies each term of the series, and its sum is not then infinite, as we have seen indeed in the particular case of the first integration in the demonstration.

Suppose $\mathrm{F}_0 + \mathrm{F}_1 + \dots + \mathrm{F}_i +$ is the series into which $\mathrm{F}(\mu, \omega)$ can be expanded. Then[*]

$$\mathrm{F}(\mu,\omega) = \frac{1}{4\pi} \int_{-1}^{1} \int_{0}^{2\pi} \{ P_0 \mathrm{F}'_0 + 3P_1 \mathrm{F}'_1 + \dots + (2i+1)P_i \mathrm{F}'_i + \dots \} d\mu'\, d\omega'.$$

If the number of terms in F (μ, ω) is finite, and F_i is the last, then it is evident that the terms involving $P_{i+1} \dots$ are all destroyed. But suppose the number of terms in F (μ, ω) is infinite, then, since by hypothesis F (μ ω) does not become infinite, the series must converge, and the term F_i must become infinitely small as i becomes infinitely great; and the integrated series on the right-hand will converge and not become infinite.

[*] Pratt's Mechanical Philosophy, Second Edition, p. 165.

LV. *Notice of the Meteors of the 9th and 10th of August,
1844, as witnessed at Bruges. By* THOMAS FORSTER, M.B.,
F.R.A.S.

To Richard Taylor, Esq.

DEAR SIR,

AS many hasty articles respecting the extraordinary quan-
tity of meteors seen on the 10th of this month have been
inserted in the papers, I send you the particulars of this phæ-
nomenon as observed by me at Bruges.

August 9.—Night clear except a few *wane clouds.* The an-
nual meteors began to appear tonight, but I only counted
seventeen of any considerable size.

August 10.—Night clear. I watched the meteors tonight
till near daybreak; they began to be visible at half-past 8
o'clock and continued without intermission all night, though
most plentiful between 10 and 12 o'clock. Their number was
prodigious, amounting to an average of *ninety-six per hour*, of
which I determined *about seventy per hour* to have a decided
point of convergence somewhere about *Antares* and *Scorpio*,
a circumstance which I think almost as puzzling as their pe-
riodic appearance. They were of various colours, and gene-
rally left long white trains behind them in their track, but not
such large and lasting trains as those left by the meteors of the
10th of August, 1811, which I have already described in your
Magazine for that year*.   I am Sir, yours, &c.,
August 19, 1844.   T. FORSTER.

LVI. *On the Action of the Yellow Rays of Light on Vegeta-
tion. By* ROBERT HARKNESS, *Esq.*

To the Editors of the Philosophical Magazine and Journal.
GENTLEMEN,

THE Philosophical Magazine for January contains a paper
upon the Action of Light on Vegetation by Dr. Gard-
ner, in which he states that one of the results of his conclu-
sions is, that yellow light by its influence produces the decom-
position of carbonic acid and gives rise to the formation of the
hydro-carbonaceous compound called chlorophylle. This cir-
cumstance is highly probable, for we know that the maximum
of light is in this ray, and also that the green colour of vege-
tables is dependent upon, in a great measure, the intensity of
solar light. But in the fourth paragraph of his paper it is

---

* A curious circumstance.—I find by the journals that the great meteor
seen all over England, August 18, 1783, is reported to have been also seen
all over Europe. Now I cannot find any account of it on the continent,
nor is it mentioned in any of the reports of the Palatine Society under
that day.

mentioned that "turnep *seeds* were sown, and every grain germinated in the yellow and red rays—the greenest plants were found in yellow light." This is directly in opposition to Mr. Hunt, who found in his experiments, " that the yellow and red rays destroy the vital principle in the seed." From this circumstance there appears to be either some error on the part of one of these gentlemen, or that some circumstances have affected the experiments of the one which have not influenced those of the other. Dr. Gardner supposes the difference to have arisen from the earth being *rapidly dried* in the experiments of Mr. Hunt, which he considers sufficient to retard or destroy germination.

It appears to me that Dr. Gardner has overlooked one circumstance in his experiments which seem strongly to militate against the production of chlorophylle by yellow light, or else to show that there is some error either in his experiments or in the account which he has published of them; this is, that the *seeds* of vegetables germinate most rapidly in yellow light. In the paragraph referred to he merely states this in general terms, whilst in giving the results of the other experiments in detail he mentions that *seedling plants* were operated upon. Now we know, both from the observations of Ingenhousz and Sennebier, as well as from daily experience, that the absence of solar light is one of the conditions almost necessary for the germination of seed, and consequently we should not expect that ray in which the maximum of light is found to facilitate germination, but on the contrary, as in Mr. Hunt's experiments, to retard it.

This, however, is not the only objection to which this statement is liable, for if the yellow ray be the operating cause by which carbonic acid is decomposed and chlorophylle produced, we should also expect that, so far from assisting in germination, it would exercise a highly injurious influence. We know that the presence of oxygen is necessary for the vegetation of seeds, and also that this oxygen is absorbed, and by uniting with a portion of the carbon in the seed reappears in the form of carbonic acid, a process the opposite of that which takes place when chlorophylle is produced. This change is required in order that the amylaceous principle in the seed may be rendered sufficiently soluble for the support of the germ, previous to its being able to obtain the requisite nutriment from the soil; so that, if the decomposition of carbonic acid be owing to the yellow ray, this same ray ought to be the last to produce any effect on the germination of seeds. It appears moreover that seeds as well as fully developed vegetables possess the power when deprived of light of absorbing oxygen and evolving car-

bonic acid; and to this circumstance we must ascribe the ef-
fect of the blue ray as shown by Mr. Hunt's experiments.

I should feel obliged if you would insert this in the Philo-
sophical Magazine, as it would perhaps attract the attention
of those who are engaged in experiments on this subject.

<div style="text-align:right">I am, Sir, your obedient Servant,</div>

Ormskirk, August 21, 1844.   ROBERT HARKNESS.

LVII. *Descriptive Notice of the Morro Velho Mine, Province of Minas Geraës; and on the Relations between the Structure of the Containing Rocks and the Directions of the Shoots of Gold in the Brazilian Mines.* By WILLIAM JORY HEN-WOOD, *C.E., F.R.S., F.G.S., Chief Commissioner of the Gongo Soco Gold Mines**.

THE gold mine of Morro Velho is about 48 miles north-west of the city of Ouro Preto and 10 miles south of
the town of Sabará: it has been worked by English compa-
nies for nearly sixteen years, but unsuccessfully until within
three years past.

It is situated on the flank of a mountain of considerable
elevation; to the contour of which the metalliferous mass has
an approximate parallelism, a circumstance of frequent occur-
rence in the gold formations of Brazil.

a. The rock mostly consists of a dark lead-coloured clay-
slate, and occasionally contains traces of chlorite, whilst there
are some layers of a dark brown colour, and others which are
quartzose and have a crystalline structure. The general bear-
ing of the lamination is from N.E. to S.W., and the dip is
usually towards the S.E. from $40°$ to $70°$. One series of joints
bears about E. and W., and another N. and S. (magnetic).

b. The direction, dip, and dimensions of the mass from
which the gold is extracted are very irregular, and although
it has some characters in common with metalliferous veins
(*lodes*), it has others which more nearly resemble the forma-
tions of tin ore at the Saint Ives Consols†, Rosewall Hill‡,
and Balnoon§. The accompanying ground plan is copied
from that used at the mine, and exhibits the peculiarities of
this formation more clearly than any description could do.

The most westerly part of the mine is called the Champion
Ground, and consists of an assemblage of small veins; these
unite with the little Quebra Panella, which dips N. $70°–80°$,
and is, on an average, about eighteen feet wide, although in
this respect it varies very much in different parts. Eastward

* Communicated by the Author.   ‡ Ibid. p. 237.
† Cornwall Geol. Trans., vol. v. p. 21.   § Ibid. p. 24.

this joins the great Quebra Panella, which inclines towards the S. 60°-75°, and, although of very irregular dimensions, its
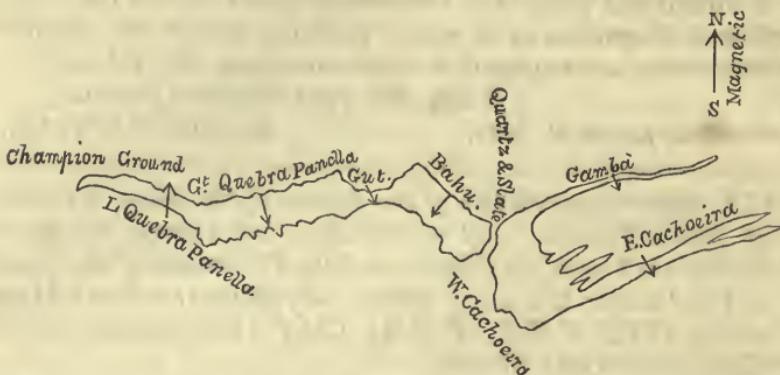
greatest breadth is not less than forty-eight feet. The Gut forms the prolongation eastward, and dips S.E., parallel to the cleavage of the contiguous slate rock: the widest parts of this are not more than eight feet in breadth, and the greater portion is much narrower. Connected with the last-named part, on the E., is the Bahu, which underlies S., and is, on an average, about twenty-four feet wide. A bed of quartzose slate, slightly charged with iron pyrites, separates the Bahu from the Western Cachoeira, which is at present unwrought and inaccessible. Two principal veins and several smaller ones branch off from the Western Cachoeira; the most northerly is the Gambà, which dips S., and is from two to eight feet in breadth; the other, called the Eastern Cachoeira or Luiza, has about the same width and inclination, and still further eastward also divides into other and smaller veins.

c. The mineral composition of the Champion Ground is quartzose slate, thinly spotted with pyrites. That of the other productive portions is, for the most part, very arsenical iron pyrites, more or less mixed with quartz and a little slaty matter, and frequently spotted with copper pyrites. Occasionally, too, isolated masses of black quartz and of copper pyrites are found, as well as cavities (*vughs*) lined with crystals of quartz and of arragonite. It is with these ingredients that the gold is mixed, and here, as in the greater number of Brazilian mines, the richer parts are usually near the upper side (*hanging wall*) of the vein; the metal is however seldom, or never, distinguishable in the stone.

The shallower part of the Eastern Cachoeira consists of earthy-brown iron ore (*gossan*), interspersed with stones of iron and copper pyrites.

d. The connexion between the metalliferous mass and the contiguous rock is by a gradual transition, the pyritous matter

being gradually replaced by quartzose and slaty substances towards the sides of the vein, much as in many Cornish mines[*]. Joints also sometimes intervene and give a deceptive appearance of distinct demarcations (*smooth walls*) between them; in such cases, however, the mineral composition is much alike on both sides of the joints[†]. On the upper side (*hanging wall*) of the great Quebra Panella the pyrites and the adjoining slate mutually penetrate each other, giving rise to a serrated appearance.

e. The metallic minerals when brought to the surface are *spalled* and *stamped*, as tin ores and the poorer copper ores are in Cornwall; and the gold is afterwards separated from the other ingredients by amalgamation.

f. During the month of December 1843, the pyrites extracted from the mine was 2168 tons, and it yielded 110 troy lbs. of gold, or about 292 grains of gold from each ton of ore[‡].

g. All the machinery is worked by water, and about 70 Europeans and 700 negroes are employed.

h. The quantity of water in the mine is inconsiderable; at about forty-two fathoms deep in the Bahu it issues from the vein at a temperature of 68°; and somewhat shallower, in the Eastern Cachoeira, at 69°.

The richer portions of the vein at Morro Velho, as in all other mines, are connected by vein-stones of a less metallic composition. There are however four well-marked enlargements of it, which respectively preserve the same relative positions, the same configuration, and indeed almost exactly the same dimensions, at every other portion of their descent yet seen, as they expose in their outcrop at the surface. They have a regular inclination of about 45° towards the E., on a line which bears 2° N. of W. (magnetic); and this regularity equally prevails, whether the dip of the vein may be parallel or oblique to the cleavage of the containing rock, whether it may be uniform or variable.

A circumstance of value to the miner, as well as of interest to the geologist, is however common to most, if not to all, of the Brazilian mines; namely, that whether the veins may be parallel, transverse, or oblique to the schistose structure of the rock, their sides (*walls*) are grooved or fluted with large striæ, which are coincident with the dips of the *shoots* of gold, both in the angles and directions of their inclinations.

These facts are most conspicuous at Morro Velho, where the gold is dispersed through pyrites, and the rock is clay-slate; at Gongo Soco, where enormous masses of gold occur

[*] Cornwall Geol. Trans., vol. v. p. 185.   [†] Ibid. p. 184.
[‡] Mining Journal, 1844, March 16th.

in manganesic oxide of iron (*jacotinga*), and the rock is schistose specular iron ore; and at Catta Preta, where the veinstone is quartz, and the country quartzose sandstone. At Gongo Soco the *shoots* dip eastward from the granitoid rocks of the Serra de Tejuco, whilst at Catta Preta they incline westward from similar rocks near Inficionada.

There is not only a remarkable similarity between these facts and the disposition of the *shoots* of ore in Cornwall and Devon*, but the striæ present a second class of phænomena, which are either less conspicuous† or altogether wanting there.

There are some remarkable analogies between the arrangement of gold in Brazil and that of tin ore in Cornwall, but I reserve their discussion until another opportunity.

The auriferous rocks of Brazil afford but few data for assigning their geological epoch; there is, however, much probability that they may eventually be found the equivalents of the metalliferous formations of the West of England.

Gongo Soco Gold Mines, June 29, 1844.   W. J. HENWOOD.

LVIII. *On Electro-Rotatory Points, and the Pyro-Electricity of Glass.* By JOSEPH READE, *Esq.,* M.D.‡

IN the year 1750 Dr. Franklin made a number of experiments to ascertain the attractions and discharges of the electric force from points, but as these points were *fixed*, such as a needle held in the hand before a conductor, or a shoemaker's punch under a pair of excited scales to represent the clouds, Dr. Franklin says " he could not account for the attractions." If the scales were electro-positive, the punch must have been electro-negative. Rotatory points were first used in 1660 by Dr. Gilbert (described in his work *De Magnete*). This instrument at present, with some little alterations, is known as Haüy's electroscope, and consists of a slender wire revolving on a pivot, and armed at both ends with gilt pith-balls. In the year 1785, a celebrated French philosopher, Coulomb, invented the balance of torsion, and turned his attention to electric points, but as they were *fixed*, like those of Franklin, the same objections remain. Indeed the only revolving points were the electric fly and the electric orrery, and in these the revolutions were erroneously attributed to recoil between the air and the electric force when discharged,—a physical impossibility. An imponderable acting on a ponderable substance is unknown in the science of statics; as well might light recoil.

---

* Cornwall Geol. Trans., vol. v. p. 193. Edin. New Phil. Journal, vol. xxii. p. 157.   † Cornwall Geol. Trans., vol. v. p. 182.
‡ Communicated by the Author, through Mr. E. W. Brayley.

Experiment 1.—Having heated and excited a glass tube by means of a silk handkerchief*, I held it parallel to a brass needle about three inches in length and one-fourth of an inch in width, and placed on a pivot with a wide and shallow cap and insulated stand; the needle began immediately to revolve on its centre of motion, and continued for fifty or sixty revolutions; it then began to oscillate, and after about twenty rounds fixed at right angles to the tube. The following diagram may give a more defined explanation :—
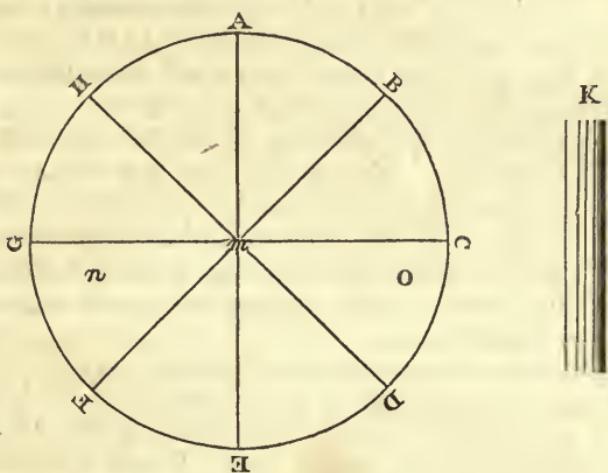
Let A B C D E F G H represent a horizontal plane, through which the needle or brass bar A E revolves on the fulcrum or centre of motion *m*. When the excited glass tube is held parallel at K, the arm of the bar A E is powerfully attracted to C, where it becomes like the glass tube, electro-positive; consequently it is now repelled to E with a force equal to that of attraction. By this motion E has taken the place of A, and is become, by giving out electricity, electro-negative, and is therefore attracted. Thus continual revolutions are produced so long as the electric force, viz. attraction, repulsion, and moment, are together equal to 180°; the velocity being in a direct ratio to the force of attraction and repulsion, so that A : C :: E : C, &c., $\dfrac{C\,E}{A} = C$. After some time the attraction and repulsion A C, E C decrease, and being together less than 180°, oscillation takes place, say within the sector B *m* D, continuing according to a decreasing series. Biot and Coulomb have established the law, that "the loss of electricity is equal in equal time," and equal to the force of attraction and

* This experiment can be conveniently made with a warm excited tumbler or wine-glass; thus ⌊⌋ ⊤

repulsion. The oscillations are then explained. At C the bar becomes electro-positive and at B and C electro-negative; final-ly, the bar fixes at G *m* C perpendicular to K. Although sta-tionary, we are not to infer that all electricity has been dissi-pated, for to my surprise, on holding the point of my fore finger at O perpendicular to the bar, C was repelled to B. Here my finger must have become electro-negative, the same as the needle; when my finger was held at *n* the bar was attracted; from this we are not to infer any electro-polarity, for my finger must have changed its electricity with the distance; on substi-tuting an excited stick of sealing-wax instead of the glass tube, I found that the needle was likewise repelled, according to the re-ceived opinion, that excited sealing-wax is always electro-nega-tive; in this case the bar and also my finger must be electro-positive by induction. However, I shall not stop to theorize on these interesting facts, for facts in our present state of ex-perimental philosophy are more valuable than theories; for-tunately inductive reasoning is gaining ground, and the wild dreams of the greatest men melting away, and only an *ipse dixit* is at a discount.

For some time I have used this brass needle as a revolving electrometer for measuring relative electricities, insensible to either the gold-leaf or torsion electroscopes, for it is a mis-nomer to call them by any other name; I used it in the follow-ing manner:—Having surrounded the insulated stand with a graduated semicircle, it was placed parallel to the substance to be examined, and then, if the electricity was very weak, the arm was moved to 45°; on removing the hand the needle os-cillated, and the number of oscillations indicated the relative force. When the bar was stationary, by repelling with the point of my finger I could ascertain the most minute quantity. Let us now apply this revolving electrometer * to an entirely new subject; — the pyro-electricity of glass, considerably greater than that of the tourmaline.

Experiment 2.—*On the Pyro-Electricity of Glass.*

October 10, 1844.—When the thermometer was at 64° with the door of the parlour open, I placed the needle on its insu-lated stand on the hob, and then placed two tumblers, one at each side, so as to allow the lever to revolve; the needle immediately began to revolve at the rate of sixty or a hun-dred, according to the heat, in a minute, and continued the motion as long as the fire was sufficient. When the tum-blers were removed, the revolutions decreased and finally

* This instrument may be seen at Messrs. Watkins and Hill's, Charing Cross.

stopped. Having shown this novel experiment to some scientific friends, it was suggested that the electricity might be produced by currents of heated air circulating around the glasses. To ascertain this, I blew with a bellows on the glasses without stopping the rotations: these revolutions are explained by the before-mentioned diagram, and arise from attraction and repulsion. To show beyond doubt that the phænomenon was electric, I removed one of the glasses and brought the arm of the needle so as to touch the other glass; it was immediately repelled, and both arms stood parallel to a tangent, sometimes one arm being somewhat attracted and then repelled. A heated glass tube attracted either arm. When the doors were shut the revolutions decreased; this did not arise from any draught up the chimney, but from pure air being more favourable than stagnant and warm air, in some measure deprived of its oxygen. I now dipped my tumblers in water, and on replacing them the velocity was increased to one hundred in a minute; filling them with cold or hot water made no difference. Did the evaporation increase the electricity?

I also caused the needle to revolve between two cylinders of china, which I afterwards powerfully excited with a silk handkerchief. These experiments, if confirmed, open a new and wide field in the science of electricity, which I hope may be cultivated with success. I have come to the conclusion, after numerous experiments, that all bodies reflecting radiant heat become electric, even different coloured cloths; and consequently that *radiant heat and electricity are mutually convertible.* When the needle was placed in the focus of the iron-curved side of the grate, it became electric and revolved. I found also that the needle revolved in the focus of a polished mirror, opposite a fire or another mirror, with a heated ball, similar to the late Sir John Leslie's experiment. I am almost sanguine enough to believe that a very delicate needle may be made magnetic by radiant heat, such as that of the sun. I believe Mrs. Somerville advanced the same opinion.

On returning from the Polytechnic Institution*, highly pleased with Mr. Armstrong's hydro-electric machine, I placed a bright copper kettle, more than half-full of distilled water, on the fire, insulated by four black bottles; when rapidly boiling I placed the revolving electrometer near it on the hob, the revolutions immediately began; after continuing for some time I took off the cover, still the revolutions proceeded, but somewhat decreased. Were I to venture an opinion

---

* The author begs leave to thank Prof. Bachoffner and the gentlemen of that Institution, for their courtesy in permitting him to experiment with their powerful apparatus.

after the able experiments and inferences of Dr. Faraday and
Mr. Armstrong, I should attribute the electricity to the fric-
tion of the water against the sides of the boiler, caused by
rapid currents of steam passing from the bottom, aided by im-
mense pressure and heat. If it were the steam carrying water,
surely the greatest electrical force should be at the exit, and
not at the sides ; the steam may be a carrier and give shock.

To show the conducting power of steam, at-
tach a pith-ball by a silk thread and wax to
an excited glass tube.   On bringing this
near a lighted candle the electricity is dissi-
pated.   The celebrated philosopher, M.
Arago, states that this is occasioned by the
conical blaze, whereas it arises from the va-
pour of the candle.
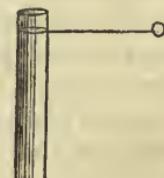
In my next communication to your valuable Journal I shall
make known some perfectly new, and I hope interesting, ex-
periments on the pyro-electricity of the glass cover of the bi-
nacle compass produced by the solar rays, giving a new view
of that deflection of the needle which was attributed to local
attraction.   Humanity makes this a subject of great interest,
and therefore I shall give the experiments much attention.

LIX. *Description of a Simple Method of Magnetizing Steel
Bars.   By* P. Elias *of Haarlem**.

A T the present day, when the possession of powerful steel
magnets must be desirable to experimentalists, it will
perhaps not be without interest to many to become acquainted
with the method by which the smallest needle as well as the
heaviest steel bar can be instantly magnetized to saturation
with the greatest facility.

The method hitherto employed is that of the double passing,
discovered by Knight and improved by Duhamel and Mitchell.
Several persons have proposed the employment of electro-
magnets for this purpose, others suspending the steel bar to be
magnetized at a red heat to the pole of an electro-magnet, and
allowing it to cool in that position.   The first method is in-
convenient, as it is not possible to move electro-magnets about
so freely as steel magnets ; the latter, as every one knows, is
without effect ; consequently the old method has been retained.
This however has the inconvenience, that in the first place it
is necessary to be well acquainted with the manipulation of
magnetizing ; and secondly, that you must already possess that

which is to be made, viz. highly powerful magnets, which is rarely the case.

With my method I require nothing more than what every experimentalist possesses, and may be had everywhere, a powerful voltaic pair and some copper wire. From twenty-two to twenty-five feet of well-isolated copper wire are wound round to form a hollow, very short, but very thick cylinder (see woodcut). A strong current is passed through the wire,
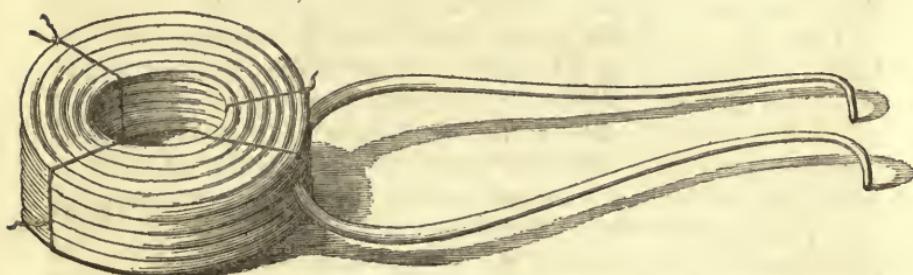
and the steel bar to be magnetized is placed in the cylinder, in which it is moved up and down to the very ends. When the central portion of the steel bar again occupies the cylinder, the circuit is opened, and the bar, which is now perfectly magnetized, withdrawn. When the bar is curved in the form of a horse-shoe, it is well to close it with its keeper during the magnetizing, and when a straight one, to provide it at top and bottom with a piece of soft iron. The wire which I employ is $\frac{1}{5}$th of an inch in thickness. The thickness of the wire is not at all indifferent; the same effect it is true may be produced with a thinner one, but then it requires a battery of greater intensity. The voltaic apparatus which I make use of is a single platinum and zinc pair arranged on Grove's principle; it has $\frac{1}{3}$rd square foot active platinum surface; the resistance which the current meets with in this battery is equal to that of a clean copper wire, 1 millimetre in diameter and $\frac{2}{3}$rds of a metre in length*. The experiments however succeed perfectly well, even when the battery presents a twice or three times greater resistance. My hollow cylinder is 1 inch high, the bore is nearly $1\frac{1}{2}$ inch in diameter, and its sides likewise $1\frac{1}{2}$ inch thick.

To test the efficiency of my magnetizing cylinder, a friend

* Since in Grove's battery the smallness of the resistance depends not merely on the size of the metallic surface, but likewise considerably on the goodness of the porous earthen cell, which cannot be determined, I have conceived it to be possible to indicate the power of the battery independent of the resistance only in this manner.

of mine had a steel horse-shoe made, weighing thirty-four pounds, of one piece. It was magnetized to saturation by one single passage through a cylinder constructed purposely for this experiment; and a second magnet, after having been placed reversed in the cylinder, proved sufficient to change the poles without having lost its suspending power. Besides the facility and rapidity of this method, it has moreover the advantage, that a bar magnetized in this manner cannot possibly have intermediate poles; and if it previously had possessed any they disappear instantly in the magnetizing cylinder.

This mode is nothing more than the double passing of Duhamel or Mitchell, only by means of galvanism, and far more powerful, easy and certain. As in the double passing the opposite poles of the magnet employed must be kept close together, so as to exert successively their greatest action upon each small part of the bar to be magnetized, in like manner I make my cylinder quite short, that each portion of the bar may experience the entire force of the voltaic element.

My friend Dr. Munnich publicly repeated my experiments with the magnetizing cylinder at a meeting of the Utrecht Society of Naturalists; and the Faculty of the Physical Sciences which was present was convinced of the efficiency of the method. On this occasion he increased considerably the intensity of some very powerful bars belonging to the collection, and which had been magnetized by Knight himself, reversed their poles by a single passage, &c.

Haarlem, March 7, 1844.

LX. *Remarks relative to the Prevention of Explosions in Collieries. By* J. D. STAGG, *Esq.*

To the Editors of the Philosophical Magazine and Journal.
    GENTLEMEN,

THE recent and very lamentable accident at Haswell Colliery (near Durham), whereby no less than ninety-five of our fellow-creatures were hurried into eternity, must occasion but one universal feeling of sorrow that such awful catastrophes cannot yet be prevented.

When we further consider the consequent misery of the surviving relatives, and the prospect that we are not in possession of any means of effectually guarding against the recurrence of such a fearful loss of human life, it should stimulate us to increased care in the working of collieries, and to endeavours to prevent the possibility of such events.

I am not practically acquainted with the winning of coal, but I beg to bring before the attention of better informed par-

ties an idea that may be of some value, and, possibly, in abler hands, may be rendered available.

It appears that though the safety lamp possesses many merits, and has doubtless been of immense service, yet, under certain circumstances, it fails to offer perfect security; and this has led me to imagine that the light produced by a current of galvanic electricity might be applicable.

This could be done by having the wires from a galvanic battery conducted into the middle of a strong glass tube, in the usual way of obtaining a constant electrical light. All access to external air or inflammable gases might be prevented by carefully and hermetically sealing the parts where the wires enter, and, on the battery being charged, a sufficient and safe light would in all probability be obtained. The cost of such a method would not be so great as the present plan of Davy lamps and candles, with all the cost of repairs, &c., and the immense loss of property consequent on an explosion. Various wires, all proceeding from the battery, could be carried in different directions so as to light the whole mine, and even the most dangerous parts, at the same time.

The light obtained could also, at all events, be made, by means of a sufficiently powerful battery, at least equal to that of a Davy lamp.

The only objection, that of the liability of the glass tube to be broken by the dropping of water, &c. from the roof, could be obviated by placing a shield over such a glass tube, and still more effectually by inclosing the tube in a second one, which would be destroyed without affecting the interior one; for which suggestion I am indebted to Dr. White, an eminent physician in Newcastle.

I had the happiness of meeting Prof. Liebig in Mr. Richardson's laboratory in Newcastle, a few days after the above accident, and mentioned the suggestion to him, of which he expressed a favourable opinion, but said that he *believed* it had been proposed many years ago.

The idea, however, being new to myself, I hope you may so far approve of the same as to give the above few remarks a place in your valuable Journal. When meeting the eyes of the parties more conversant with electrical phænomena, it may be worked out so as to become, in their hands, an effectual means of saving the lives of our miners. I beg to subscribe myself,

Yours respectfully,

Newcastle-on-Tyne, October 7, 1844.   J. D. STAGG.

LXI. *Investigation of the Transformation of certain Elliptic Functions. By* ARTHUR CAYLEY, *Esq.*, *B.A.*, *F.C.P.S.*, *Fellow of Trinity College, Cambridge.*

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

I should be obliged if you would do me the favour of insert-ing the following investigation of the transformation of elliptic functions, which appear to me completely to demon-strate that no limitations are required in Jacobi's conclusions (see Phil. Mag. S. 3. vol. xxii. p. 358 ; xxiii. p. 89). I should not have resumed the subject had not the method employed seemed to me to possess some independent interest.

I remain,

Your obedient Servant,

29 York Terrace, October 10, 1844.   A. CAYLEY.

The function sin *a m u* (ϕu for shortness) may be expressed in the form

$$\left. \begin{aligned} \phi u = u \, \Pi \left(1 + \frac{u}{2\,m\,\mathrm{K} + 2\,m'\,\mathrm{K'}\iota} \right) \\ \div \Pi \left(1 + \frac{u}{2.\,m\,\mathrm{K} + (2\,m'+1)\,\mathrm{K'}\iota} \right) \end{aligned} \right\}, \quad . \quad . \quad (1.)$$

where *m, m'* receive any integer, positive or negative, values whatever, omitting only the combination $m = 0$, $m' = 0$ in the numerator (Abel, *Œuvres*, t. i. p. 212, but with modifications to adapt it to Jacobi's notation ; also the positive and negative values of *m, m'* are not collected together as in Abel's formulæ). We deduce from this

$$\left. \begin{aligned} \frac{\phi (u+\theta)}{\phi \theta} = \Pi \left(1 + \frac{u}{2\,m\,\mathrm{K} + 2\,m'\,\mathrm{K'}\iota + \theta} \right) \\ \div \Pi \left(1 + \frac{u}{2m\mathrm{K} + (2m'+1)\mathrm{K'}\iota + \theta} \right) \end{aligned} \right\}. \quad . \quad . \quad (2.)$$

Suppose now $\mathrm{K} = a\,\mathrm{H} + a'\,\mathrm{H'}\iota$, $\mathrm{K'} = b\,\mathrm{H} + b'\,\mathrm{H'}\iota$, *a, b, a', b'* integers, and $a\,b' - a'\,b$ a positive number (*ν*). Also let $\omega = f\,\mathrm{H} + f'\,\mathrm{H'}\iota$; *f, f'* integers such that $a\,f' - a'f$, $b\,f' - b'f$, *ν*, have not all three any common factor. Consider the ex-pression

$$\upsilon = \frac{\phi u \,.\, \phi (u + 2\,\omega) \dots \phi (u + 2(\nu - 1)\omega) \,.}{\phi (2\,\omega) \dots \phi (2(\nu - 1)\,\omega)}, \quad . \quad . \quad (3.)$$

from which

$$\left. \begin{aligned} \upsilon = u \, \Pi \left(1 + \frac{u}{2\,m\,\mathrm{K} + 2\,m'\,\mathrm{K'}\iota + 2\,r\,\theta} \right) \\ \div \Pi \left(1 + \frac{u}{2m\mathrm{K} + (2m'+1)\mathrm{K'}\iota + 2r\theta} \right) \end{aligned} \right\}. \quad . \quad . \quad (4.)$$

where ν extends from 0 to $\nu - 1$ inclusively, the single combination $m = 0$, $m' = 0$, $r = 0$ being omitted in the numerator. We may write

$$m\,\mathrm{K} + m'\,\mathrm{K'}_{\scriptscriptstyle\prime} + r\,\theta = \mu\,\mathrm{H} + \mu'\,\mathrm{H'}_{\scriptscriptstyle\prime},$$

μ, μ' denoting any integers whatever. Also to given values of μ, μ' there corresponds only a single system of values of m, m', θ. To prove this we must show that the equations

$$m\,a + m'\,b + r\,f = \mu,$$
$$m\,a' + m'\,b' + r\,f' = \mu,$$

can always be satisfied, and satisfied in a single manner only. Observing the value of ν,

$$\nu\,m + r\,(b'f - bf') = \mu\,b' - \mu'\,b;$$

and if ν and $b'f - bf'$ have no common factor, there is a single value of r less than ν, which gives an integer value for m. This being the case, $m'\,b$ and $m'\,b'$ are both integers, and therefore, since b, b' have no common factor (for such a factor would divide ν and $b'f - bf'$), m' is also an integer. If, however, ν and $b'f - bf'$ have a common factor c, so that $\nu = ab' - a'b = c\phi$, $b'f - bf' = c\,\phi$; then $(af' - a'f)\,b' = c.\,(\phi'f - \phi'f)$, or since no factor of c divides $af' - a'f$, c divides b', and consequently b. The equation (7.) may therefore be divided by c. Hence, putting $\dfrac{\nu}{c} = \nu_{\scriptscriptstyle\prime}$, we may find a value of r, $r_{\scriptscriptstyle\prime}$ suppose, less than $\nu_{\scriptscriptstyle\prime}$, which makes m an integer; and the general value of r less than ν which makes m an integer, is $r = r_{\scriptscriptstyle\prime} + s\,\nu_{\scriptscriptstyle\prime}$, where s is a positive integer less than c. But m being integral, $b\,m'$, $b'\,m'$, and consequently $c\,m'$ are integral; we have also

$$\nu\,m' + (r_{\scriptscriptstyle\prime} + s\,\nu_{\scriptscriptstyle\prime})\,.\,(af' - a'f) = a\,\mu' - a'\,\mu;$$

or dividing by $\nu_{\scriptscriptstyle\prime}$,

$$c\,m') + s\,.\,(af' - a'f) = 1$$

an integer, where c and $af' - a'f$ are prime to each other. Hence there may be found a single value of s less than c, giving an integer value for m'. Hence in every case there is a single system of values of m, m', r, corresponding to any assumed integer values whatever of μ, μ'. Hence

$$\left.\begin{aligned}
\mathrm{U} &= u\,\Pi\left(1 + \frac{u}{2\,\mu\,\mathrm{H} + 2\,\mu'\,\mathrm{H'}_{\scriptscriptstyle\prime}}\right)\\
&\div \Pi\left(1 + \frac{u}{2\,\mu\,\mathrm{H} + (2\,\mu' + 1)\,\mathrm{H'}_{\scriptscriptstyle\prime}}\right) = \phi_{\scriptscriptstyle\prime}\,u.
\end{aligned}\right\}\;.\;(5.)$$

$\phi_{\scriptscriptstyle\prime}\,u$ being a function similar to $\phi\,u$, or $\sin am\,u$, but to a different modulus, viz. such that the complete functions are H, $\mathrm{H'}$ instead of K, $\mathrm{K'}$. We have therefore

$$\phi_l u = \frac{\phi u \cdot \phi (u + 2\omega) \dots \phi (u + 2(\nu - 1)\omega)}{\phi 2\omega \dots \phi 2(\nu - 1)\omega}. \quad (6.)$$

Expressing ω in terms of K, K', we have $\nu\,\mathrm{H} = b'\mathrm{K} - a'\mathrm{K}'\iota$, $-\nu\,\mathrm{H}'\iota = b\,\mathrm{K} - a\,\mathrm{K}'\iota$, and $\therefore \nu\omega = (b'f - bf')\,\mathrm{K} - (a'f - af')\,\mathrm{K}'\iota$. Let g, g' be any two integer numbers having no common factor, which is also a factor of ν, we may always determine a, b, a', b', so that $\nu\omega = g\,\mathrm{K} - g'\mathrm{K}'\iota$. This will be the case if $g = b'f - bf'$ $g' = a'f - af'$. One of the quantities f, f' may be assumed equal to 0. Suppose $f' = 0$, then $g = b'f$, $g' = a'f$; whence $ag - bg' = \nu f$. Let k be the greatest common measure of g, g', so that $g = kg_l$, $g' = kg'_l$; then, since no factor of k divides ν, k must divide f, or $f = kf_l$, but $g_l = b'f_l$, $g'_l = af'$, and a', b' are integers, or f must divide g, g'; whence $f_l = 1$, or $f = k$. Also $ag_l - bg'_l = \nu$, where g_l and g'_l are prime to each other, so that integer values may always be found for a and b; so that in the equation (1.),

$$\omega = \frac{g\,\mathrm{K} - g'\mathrm{K}'\iota}{\nu}, \quad \dots \quad (7.)$$

g, g' being any integer numbers, such that no common factor of g, g' also divides ν.

The above supposition, $f' = 0$, is, however, only a particular one, omitting it, the conditions to be satisfied by a, b, a', b', may be written under the form

$$\left.\begin{array}{l} a b' - a' b = \nu, \\ a g - b g' \equiv 0 \;[\mathrm{mod.}\,\nu], \\ a' g - b' g' \equiv 0 \;[\mathrm{mod.}\,\nu], \end{array}\right\} \quad \dots \quad (8.)$$

to which we may join the equations before obtained,

$$\left.\begin{array}{l} \nu\,\mathrm{H} = b'\,\mathrm{K} - a'\,\mathrm{K}'\iota, \\ -\nu\,\mathrm{H}'\iota = b\,\mathrm{K} - a\,\mathrm{K}\iota, \end{array}\right\} \quad \dots \quad (9.)$$

which contain the theory of the modular equation. This, however, involves some further investigations, which are not sufficiently connected with the present subject to be attempted here.

LXII. *On Polarization and Double Refraction.* By R. Moon, *M.A., Fellow of Queen's College, Cambridge, and of the Cambridge Philosophical Society*[*].

IN a paper published in this Journal some months ago[†], I endeavoured to give a popular explanation of the phænomena of diffraction, and I attempted to show that those phænomena may be accounted for on the hypothesis, that the waves composing common light consist of recurring cycles of waves, the individuals of each cycle being related in a certain manner to each other, and occurring at regular finite inter-

vals. On the present occasion I shall endeavour to point out a consequence of the latter part of this hypothesis which has recently suggested itself to me.

Dr. Young, in his Theory of Reflexion, takes for granted that the waves composing common light occur in uninterrupted succession. But if we make a careful application of the process of reasoning which he has adopted to the case in which the waves are supposed to occur at finite intervals, we shall find that if a series of such waves be reflected at a plane surface, the reflected light will not consist of continuous waves, but the surface of each reflected wave will be intersected by dark bands, in which there will be no vibration. These bands will be parallel to each other and to the intersection of the incident waves with the reflecting surface, and their breadth will at first be exactly equal to the interval between two waves of the incident light. We say at first, for after reflection the broken surface of the wave will *tend* to re-unite so as to form a continuous surface, though this continuity will never be perfectly effected, so that the reflected waves will always consist of parallel bands of varying intensity. If the light be again reflected in a plane perpendicular to the plane of first reflexion, we shall, in like manner, have the surface of the waves twice reflected, intersected by two series of bands perpendicular to each other. Hence it is plain that if the intervals between the waves of the incident light bear any considerable proportion to the breadth of the waves, very little light will be transmitted after the second reflexion; and the light so transmitted will be different in character, both from common light and from light only once reflected; the waves being discontinuous in every direction, may not this discontinuity be such as to prevent vision? and is it not possible that the change in the nature of the reflected waves which we have endeavoured to point out may constitute polarization?

If we address ourselves to the case of refraction, we shall be led to similar results. But it should be observed that, as in the case of refraction, the breadth of the bands will not be equal to the interval between two waves, but will be equal to $\frac{\sin i'}{\sin i}$ multiplied into that interval. Hence we see that in the case of refraction the discontinuity of the wave will be sooner repaired than in the case of reflexion, or the polarization will be *less perfect*. The case of double refraction presents great, if not insuperable, difficulties, but these we will not altogether despair of removing. It will be observed that the polarization of which we have spoken must always take place in a plane perpendicular to the intersection of the incident waves with the

reflecting or refracting surface. Hence if a series of parallel plane waves be transmitted through a doubly refracting crystal, there can be none of that polarization in opposite planes of the incident and emergent rays, of which so much has been said. Although unable to support myself by any reference to the results of actual experiment, I would with all humility beg leave to doubt whether this polarization of opposite planes of the emergent rays, corresponding to the same incident pencil of parallel rays, do really obtain. No fact in physical or plane optics can be more incontestable than this, that whenever a pencil of parallel rays is incident on the surface of a refracting body, bounded by parallel planes, the emergent pencil, whatever be its course within the medium, must be parallel to the incident rays. Yet in a work so universally received as the treatise on Light of Sir John Herschel, we find it broadly stated, that if a series of parallel waves be incident on a doubly refracting crystal, bounded by parallel surfaces, the two unequal rays will be inclined to one another. This *may* be the fact, though certainly if so it militates vitally against the undulatory doctrine of reflexion, since upon that doctrine, as I conceive, it is impossible to hold any but a contrary opinion ; but it *is* possible that this apparent fact may be resolved as follows :—Assuming that the emergent rays are always parallel to the incident, it would result that the two pencils of rays, by which any small object is made visible through a doubly refracting crystal, arise from two radically distinct pencils of the incident light, the inclination of which to each other would be exactly the same as that of the two emergent pencils ; and this may possibly be the reason why it came to be believed that the emergent rays corresponding to a pencil of parallel waves were inclined to each other.

That this is the true state of the case, in the absence of experiment it is impossible to decide, but assuming it to be so, it is easy to see how the further mistake should have crept in, of supposing the two emergent pencils arising from the same incident pencil to be polarized in different planes. The emergent pencils, by which a small object is made visible through a doubly refracting crystal, are due to different pencils of the incident light ; the plane of polarization of each emergent pencil will be perpendicular to the intersection of the waves composing the corresponding pencil of incident light with the refracting surface ; therefore the two planes will necessarily be inclined to each other, but they will not necessarily be at right angles.

LXIII. *On the Variable Elements of a Disturbed Planet, and the Equations of its Motion on the Plane of the Orbit.* By *the* Rev. BRICE BRONWIN*.

THE following method of determining the elements of a disturbed planet, based upon a theorem of Lagrange, leads to a useful result. R being the perturbating function, the well-known equations of the planet's motion are

$$\frac{d^2x}{dt^2} + \frac{\mu x}{r^3} + \frac{dR}{dx} = 0, \quad \frac{d^2y}{dt^2} + \frac{\mu y}{r^3} + \frac{dR}{dy} = 0, \\ \frac{d^2z}{dt^2} + \frac{\mu z}{r^3} + \frac{dR}{dz} = 0. \quad \Bigg\} \quad \cdot \quad \text{(A.)}$$

Let x, y, z be determined from

$$\left(\frac{d^2x}{dt^2}\right) + \frac{\mu x}{r^3} = 0, \quad \left(\frac{d^2y}{dt^2}\right) + \frac{\mu y}{r^3} = 0, \quad \left(\frac{d^2z}{dt^2}\right) + \frac{\mu z}{r^3} = 0. \quad \cdot \cdot \text{(a.)}$$

These values of x, y, z will satisfy (*a*.), however the elements may vary, since t alone varies in $\left(\frac{d^2x}{dt^2}\right)$, &c. They may therefore be made to satisfy (A.). But we have six quantities to determine and only three equations: we must then assume three conditions; let them be

$$\delta x = 0, \quad \delta y = 0, \quad \delta z = 0, \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \text{(b.)}$$

δ denoting the variation of the elements only. Then

$$\frac{dx}{dt} = \left(\frac{dx}{dt}\right), \text{ &c., } \quad \frac{d^2x}{dt^2} = \left(\frac{d^2x}{dt^2}\right) + \frac{\delta dx}{dt^2}, \text{ &c.}$$

Put these values in (A.) and subtract (*a*.), there result

$$\frac{\delta dx}{dt^2} + \frac{dR}{dx} = 0, \quad \frac{\delta dy}{dt^2} + \frac{dR}{dy} = 0, \quad \frac{\delta dz}{dt^2} + \frac{dR}{dz} = 0, \cdot \cdot \cdot \text{(c.)}$$

where d refers to the variation of t only, δ that of the elements.

If Δx, Δy, Δz denote variations indeterminate, and independent one of another, we easily form from (*b*.) and (*c*.) the following, which is equivalent to (*b*.) and (*c*.), and contains the whole solution of the problem:

$$\Delta x \frac{\delta dx}{dt} - \delta x \frac{\Delta dx}{dt} + \Delta y \frac{\delta dy}{dt} - \delta y \frac{\Delta dy}{dt} \\ + \Delta z \frac{\delta dz}{dt} - \delta z \frac{\Delta dz}{dt} + \Delta R \, dt = 0. \quad \Bigg\} \quad \cdot \cdot \text{(B.)}$$

Leaving out $\Delta R \, dt$, this is independent of t. For since

$$\frac{d\Delta x}{dt} = \frac{\Delta dx}{dt}, \quad \frac{d\delta x}{dt} = \frac{\delta dx}{dt}, \text{ &c.,}$$

* Communicated by the Author.

the whole vanishes when we differentiate Δx, δx, Δy, δy, &c. for t.   Also

$$\frac{d\,\delta\,dx}{d\,t^2} = \frac{\delta\,d^2x}{d\,t^2} = -\mu\,\delta\left(\frac{x}{r^3}\right) \text{ by } (a.) = 0 \text{ by } (b.);$$

and the same of $\dfrac{d\,\delta\,dy}{d\,t^2}$, $\dfrac{d\,\delta\,dz}{d\,t^2}$.   And lastly, $\dfrac{d\,\Delta\,dx}{d\,t^2}$, &c. are multiplied by $\delta x = 0$, &c.   The variation relative to t being nothing, it is independent of t.

The quantities x, $\dfrac{d\,x}{d\,t}$, y, $\dfrac{d\,y}{d\,t}$, &c. are composed of terms of the form $A\sin(i\,n\,t+m)$, $B\cos(i\,n\,t+m)$; their variations therefore will contain terms of the form $A\,t\sin(i\,n\,t+m)$, $B\,t\cos(i\,n\,t+m)$, multiplied by δn.   And by substitution (B.) will be of the form

$$M+Nt+Pt^2+\Delta R\,dt = 0. \quad . \quad . \quad . \quad (d.)$$

But we may make t anything or nothing without altering the value of the three first terms of $(d.)$; therefore $N=0$, $P=0$.   Hence if we make $t=0$ where it appears in the co-efficients, or leave out those terms, the final result will not be affected.   And hence if we use $\displaystyle\int n\,dt$ instead of $n\,t$ for the mean longitude and consider n a function of the time, the result will be the same; for the variations δx, &c. will only differ from their former values by wanting the terms containing t in their coefficients.

Let i be the inclination, θ the longitude of the node on the fixed plane, ϑ the same longitude on the plane of the orbit, having a fixed origin on it, in which case it is easy to see that $d\vartheta = \cos i\,d\theta$.   Also let ξ and η be the rectangular co-ordinates on the plane of the orbit, the axis of ξ passing through the origin of ϑ.   Transforming by known methods from the system (x, y, z) to that in which x lies in the line of the nodes, then to another in which y lies in the plane of the orbit, and lastly to the system (ξ, η), we find

$$x = A\xi + B\eta,\ y = C\xi + D\eta,\ z = E\xi + F\eta, \quad . \quad (1.)$$

$A = \cos\theta\cos\vartheta + \cos i\sin\theta\sin\vartheta$, $B = \cos\theta\sin\vartheta - \cos i\sin\theta\cos\vartheta$,
$C = \sin\theta\cos\vartheta - \cos i\cos\theta\sin\vartheta$, $D = \sin\theta\sin\vartheta + \cos i\cos\theta\cos\vartheta$,
$$E = -\sin i\sin\vartheta,\ F = \sin i\cos\vartheta.$$

If we differentiate these last, making $d\vartheta = \cos i\,d\theta$, and compare the values of $d\,A$, $d\,E$; $d\,B$, $d\,F$; $d\,C$, $d\,E$; $d\,D$, $d\,F$; we shall see that

$$\left.\begin{array}{ll} d\,A = \tan i\sin\theta\,d\,E, & d\,B = \tan i\sin\theta\,d\,F, \\ d\,C = -\tan i\cos\theta\,d\,E, & d\,D = -\tan i\cos\theta\,d\,F. \end{array}\right\} \quad . \quad (2.)$$

And if in $x^2+y^2+z^2=\xi^2+\eta^2=r^2$ we substitute for x, y, and z their values from (1.), it will become identical. Equaling therefore the coefficients of ξ^2, $\xi\eta$ and η^2 to nothing in the result, we have

$$A^2+C^2+E^2=1, \quad B^2+D^2+F^2=1, \quad AB+CD+EF=0. \quad (3.)$$

These may be proved by putting for A, B, C, &c. their values given above.

Differentiate the two first of (3.), and

$$A\,d\,A+C\,d\,C+E\,d\,E=0, \quad B\,d\,B+D\,d\,D+F\,d\,F=0.$$

These by (2.) will give

$$(\tan i \sin \theta \, A - \tan i \cos \theta \, C + E)\, d\,E = 0,$$
$$(\tan i \sin \theta \, B - \tan i \cos \theta \, D + F)\, d\,F = 0.$$

Divide the first of these by $d\,E$, then multiply by $d\,F$; divide the second by $d\,F$, then multiply by $d\,E$, the results by (2.) will be

$$\left.\begin{array}{l} A\,d\,B+C\,d\,D+E\,d\,F=0, \quad B\,d\,A+D\,d\,C+F\,d\,E=0, \\ A\,d\,A+C\,d\,C+E\,d\,E=0, \quad B\,d\,B+D\,d\,D+F\,d\,F=0, \end{array}\right\} \quad (4.)$$

the two last being added from above.

Since

$$\frac{d\,x}{d\,t} = A\frac{d\,\xi}{d\,t} + B\frac{d\,\eta}{d\,t},$$

d denoting the variation of t only. Therefore

$$\frac{\delta dx}{dt} = A\frac{\delta d\xi}{dt} + B\frac{\delta d\eta}{dt} + \delta A\frac{d\xi}{dt} + \delta B\frac{d\eta}{dt}.$$

And

$$\Delta x = A\Delta\xi + B\Delta\eta + \xi\Delta A + \eta\Delta B.$$

Multiply the two last; there results

$$\begin{aligned}
\Delta x \frac{\delta dx}{dt} &= A^2 \Delta \xi \frac{\delta d\xi}{dt} + AB\Delta\xi\frac{\delta d\eta}{dt} + A\delta A\Delta\xi\frac{d\xi}{dt} \\
&+ A\delta B\Delta\xi\frac{d\eta}{dt} + AB\Delta\eta\frac{\delta d\xi}{dt} + B^2\Delta\eta\frac{\delta d\eta}{dt} + B\delta A\Delta\eta\frac{d\xi}{dt} \\
&+ B\delta B\Delta\eta\frac{d\eta}{dt} + AA\Delta\xi\frac{\delta d\xi}{dt} + BA\Delta\xi\frac{\delta d\eta}{dt} + \Delta A\delta A\xi\frac{d\xi}{dt} \\
&+ \Delta A\delta B\xi\frac{d\eta}{dt} + AA B\eta\frac{\delta d\xi}{dt} + B\Delta B\eta\frac{\delta d\eta}{dt} + \Delta B\delta A\eta\frac{d\xi}{dt} \\
&+ \Delta B\delta B\eta\frac{d\eta}{dt}.
\end{aligned}$$

In this result change A into C, B into D, and we have the expression of $\Delta y \frac{\delta dy}{dt}$. Again, change A into E, B into F,

and we have that of $\Delta z \dfrac{\delta dz}{dt}$. Whence, if we have respect to (3.) and (4.), we find

$$\Delta x \frac{\delta dx}{dt} + \Delta y \frac{\delta dy}{dt} + \Delta z \frac{\delta dz}{dt} = \Delta \xi \frac{\delta d\xi}{dt} + \Delta \eta \frac{\delta d\eta}{dt}$$

$$+ (\Delta A \delta A + \Delta C \delta C + \Delta E \delta E) \xi \frac{d\xi}{dt} + (\Delta A \delta B + \Delta C \delta D + \Delta E \delta F) \xi \frac{d\eta}{dt}$$

$$+ (\Delta B \delta A + \Delta D \delta C + \Delta F \delta E) \eta \frac{d\xi}{dt} + (\Delta B \delta B + \Delta D \delta D + \Delta F \delta F) \eta \frac{d\eta}{dt}.$$

If in this we make Δ and δ change places, we shall have the expression of

$$\delta x \frac{\Delta dx}{dt} + \delta y \frac{\Delta dy}{dt} + \delta z \frac{\Delta dz}{dt}.$$

But by (2.),

$$\left. \begin{aligned} \Delta A \delta B + \Delta C \delta D + \Delta E \delta F &= \frac{1}{\cos^2 i} \Delta E \delta F \\ \Delta B \delta A + \Delta D \delta C + \Delta F \delta E &= \frac{1}{\cos^2 i} \Delta F \delta E \end{aligned} \right\} \quad . \quad (5.)$$

Whence by substitution, making $\dfrac{\xi d\eta - \eta d\xi}{dt} = h$, and having regard to (5.), (B.) becomes

$$\left. \begin{aligned} \Delta \xi \frac{\delta d\xi}{dt} - \delta \xi \frac{\Delta d\xi}{dt} + \Delta \eta \frac{\delta d\eta}{dt} - \delta \eta \frac{\Delta d\eta}{dt} \\ + \frac{h}{\cos^2 i} (\Delta E \delta F - \Delta F \delta E) + \Delta R \, dt = 0. \end{aligned} \right\} \quad . \quad (C.)$$

This last divides into two. Make $p = -E$, $q = F$, and we have

$$\frac{h}{\cos^2 i} (\Delta q \delta p - \Delta p \delta q) + \Delta R \, dt = 0. \quad . \quad (D.)$$

Or by a further and obvious transformation,

$$h \sin i \, (\Delta i \delta \theta - \Delta \theta \delta i) + \Delta R \, dt = 0. \quad . \quad (E.)$$

And we shall also have for the determination of the co-ordinates or elements on the plane of the orbit,

$$\Delta \xi \frac{\delta d\xi}{dt} - \delta \xi \frac{\Delta d\xi}{dt} + \Delta \eta \frac{\delta d\eta}{dt} - \delta \eta \frac{\Delta d\eta}{dt} + \Delta R \, dt = 0. \quad (F.)$$

This, from the nature of the characteristic Δ, is equivalent to

$$\frac{\delta d\xi}{dt} + \frac{dR}{d\xi} = 0, \quad \frac{\delta d\eta}{dt} + \frac{dR}{d\eta} = 0, \quad \delta \xi = 0, \quad \delta \eta = 0. \quad . \quad (e.)$$

These will give the four elements, a, e, π and ε. But with any elements whatever, constant or variable, the values of ξ

and η will satisfy

$$\left(\frac{d^2\xi}{dt^2}\right) + \frac{\mu\xi}{r^3} = 0, \quad \left(\frac{d^2\eta}{dt^2}\right) + \frac{\mu\eta}{r^3} = 0. \quad . \quad . \quad (f.)$$

Therefore from (*e.*) and (*f.*) by addition, there results

$$\frac{d^2\xi}{dt^2} + \frac{\mu\xi}{r^3} + \frac{d\,\mathrm{R}}{d\xi} = 0, \quad \frac{d^2\eta}{dt^2} + \frac{\mu\eta}{r^3} + \frac{d\,\mathrm{R}}{d\eta} = 0, \quad . \quad (\mathrm{G.})$$

which are the same as if the plane of the orbit were fixed, the result intimated at the beginning of this paper.

To effect a further transformation of (F.), let υ be the longitude on the orbit. Then $\xi = r\cos\upsilon, \quad \eta = r\sin\upsilon$;

$$\frac{d\xi}{dt} = \cos\upsilon\,\frac{dr}{dt} - r\sin\upsilon\,\frac{d\upsilon}{dt}, \quad \frac{d\eta}{dt} = \sin\upsilon\,\frac{dr}{dt} + r\cos\upsilon\,\frac{d\upsilon}{dt};$$

$$\frac{\delta d\xi}{dt} = \cos\upsilon\,\frac{\delta dr}{dt} - r\sin\upsilon\,\frac{\delta d\upsilon}{dt} - \sin\upsilon\,\delta\upsilon\,\frac{dr}{dt} - \sin\upsilon\,\delta r\,\frac{d\upsilon}{dt} - r\cos\upsilon\,\delta\upsilon\,\frac{d\upsilon}{dt};$$

$$\frac{\delta d\eta}{dt} = \sin\upsilon\,\frac{\delta dr}{dt} + r\cos\upsilon\,\frac{\delta d\upsilon}{dt} + \cos\upsilon\,\delta\upsilon\,\frac{dr}{dt} + \cos\upsilon\,\delta r\,\frac{d\upsilon}{dt} - r\sin\upsilon\,\delta\upsilon\,\frac{d\upsilon}{dt}.$$

Also $\quad \Delta\xi = \cos\upsilon\,\Delta r - r\sin\upsilon\,\Delta\upsilon, \quad \Delta\eta = \sin\upsilon\,\Delta r + r\cos\upsilon\,\Delta\upsilon.$

With these values we shall find

$$\Delta\xi\,\frac{\delta d\xi}{dt} + \Delta\eta\,\frac{\delta d\eta}{dt} = \Delta r\,\frac{\delta dr}{dt} - r\,\Delta r\,\delta\upsilon\,\frac{d\upsilon}{dt}$$

$$+ r^2\,\Delta\upsilon\,\frac{\delta d\upsilon}{dt} + r\,\Delta\upsilon\,\delta\upsilon\,\frac{dr}{dt} + r\,\Delta\upsilon\,\delta r\,\frac{d\upsilon}{dt}.$$

Making Δ and δ change places, we have the expression of

$$\delta\xi\,\frac{\Delta d\xi}{dt} + \delta\eta\,\frac{\Delta d\eta}{dt}.$$

By these values, observing that $\dfrac{d\upsilon}{dt} = \dfrac{h}{r^2}$, we shall find (F.) transformed into

$$\left.\begin{array}{c} \Delta r\,\dfrac{\delta dr}{dt} - \delta r\,\dfrac{\Delta dr}{dt} + r^2\left(\Delta\upsilon\,\dfrac{\delta d\upsilon}{dt} - \delta\upsilon\,\dfrac{\Delta d\upsilon}{dt}\right) \\[2mm] + \dfrac{2h}{r}\left(\Delta\upsilon\,\delta r - \Delta r\,\delta\upsilon\right) + \Delta\,\mathrm{R}\,dt = 0. \end{array}\right\} \quad . \quad . \quad (\mathrm{H.})$$

We shall suppose t to have such a value that υ may be equal to π, the longitude of the apse. Then

$$\frac{dr}{dt} = \frac{\mu e}{h}\sin(\upsilon - \pi), \quad \frac{\delta dr}{dt} = \frac{\mu e}{h}(\delta\upsilon - \delta\pi).$$

Also since

$$\frac{d\upsilon}{dt} = \frac{h}{r^2}, \quad \frac{\delta d\upsilon}{dt} = \frac{1}{r^2}\delta h - \frac{2h}{r^3}\delta r.$$

Putting these values in (H.), it becomes

$$\left.\begin{array}{c} \dfrac{\mu\,e}{h}(\Delta\,r\,\delta\,\upsilon - \Delta\,\upsilon\,\delta\,r) + \dfrac{\mu\,e}{h}(\Delta\,\pi\,\delta\,r - \Delta\,r\,\delta\,\pi) \\ + \Delta\,\upsilon\,\delta\,h - \Delta\,h\,\delta\,\upsilon + \Delta\,\mathrm{R}\,dt = 0. \end{array}\right\} \quad \cdot \quad \cdot \quad (g.)$$

If u be the eccentric anomaly,

$$r = a\,(1 - e\cos u), \quad \delta\,r = (1 - e)\,\delta\,a - a\,\delta\,e,$$

making $u = 0$ after taking the variations.   Also

$$\tan\frac{\upsilon - \pi}{2} = \sqrt{\frac{1 + e}{1 - e}}\tan\frac{u}{2}, \quad \delta\,\upsilon = \sqrt{\frac{1 + e}{1 - e}}\,\delta\,u + \delta\,\pi,$$

$$u - e\sin u = nt + \varepsilon - \pi, \quad (1 - e)\delta u = -\frac{3\,nt}{2\,a}\delta a + \delta\varepsilon - \delta\pi = \delta\varepsilon - \delta\pi$$

by making $t = 0$, as it has been proved we may do.   Therefore by elimination,

$$\delta\,\upsilon = \frac{(1 - e^2)^{\frac{1}{2}}}{(1 - e)^2}(\delta\,\varepsilon - \delta\,\pi) + \delta\,\pi.$$

With these values of $\delta\,r$, $\delta\,\upsilon$, $(g.)$ becomes, making

$$k = 1 - \sqrt{1 - e^2},$$

$$\left.\begin{array}{c} \dfrac{n\,a\,k}{2}(\Delta\,a\,\delta\,\pi - \Delta\,\pi\,\delta\,a) + \dfrac{n\,a}{2}(\Delta\,\varepsilon\,\delta\,a - \Delta\,a\,\delta\,\varepsilon) \\ + \dfrac{\mu\,a\,e}{h}(\Delta\,e\,\delta\,\pi - \Delta\,\pi\,\delta\,e) + \Delta\,\mathrm{R}\,dt = 0. \end{array}\right\} \quad \cdot \quad \cdot \quad (\mathrm{I.})$$

By making the coefficients of $\Delta\,a$, $\Delta\,e$, &c. separately equal to nothing, the equations (D.), (E.) and (I.) will give all the elements.

If e' be the eccentricity, π' the longitude of the apse, and ε' the epocha on the fixed plane, such as would be introduced by the integration of the differential equations relative to that plane, I find by a comparison of the values of the co-ordinates on the plane of the orbit and the fixed plane,

$$e\cos(\vartheta - \pi) = e'\cos(\theta - \pi'), \quad e\sin(\vartheta - \pi) = e'\cos i\sin(\theta - \pi');$$

and also $\varepsilon - \varepsilon' = \vartheta - \theta$ true to quantities of the order e^2 inclusive. Perhaps this last, like the two former, is strictly true.   It is evident therefore that e' and π' at least differ from e and π, and that the same values cannot be used for either set indiscriminately without the hazard of error.   I have not room to discuss this subject at length, but I wish to invite attention to it. The main object of this paper is the formulæ (G.), which I have found by a different method elsewhere.

Gunthwaite Hall, Penistone, Yorkshire,
      September 30, 1844.

LXIV. *On the Composition of Narcotine, and some of its products of Decomposition by the action of Bichloride of Platinum. By* J. BLYTH, *M.D.**

NARCOTINE has been analysed by M. Liebig, Pelletier and Regnault. From these analyses, although agreeing in the relative proportion of the constituents, different formulæ have been deduced, arising from the dissimilar atomic weights admitted by each.

The original method adopted by M. Liebig for determining the atomic weight, was by saturating a known portion of narcotine with hydrochloric acid gas, and after expelling the excess of acid in a drying apparatus by a current of air, the increase of weight of the original substance indicated the amount of acid which had entered into combination. The number found by this method was 4799 †.

The analysis gave in a hundred parts—

Carbon	65·00
Hydrogen	5·50
Nitrogen	2·51
Oxygen	26·99

The formula deduced from which was $C_{40} H_{20} N O_{12}$.

M. Regnault, in a later research on the constitution of the organic alkalies, investigated very carefully the atomic weight and composition of narcotine ‡. The plan pursued by him was the same as that employed by M. Liebig. Different portions of narcotine saturated with hydrochloric acid gas gave 5139, 5127·6, 5253 as the atomic weights. The composition after combustion expressed in 100 parts was found to be

	I.	II.	III.
Carbon	64·91	65·30	64·98
Hydrogen	5·96	5·99	5·73
Nitrogen	3·46	3·52	
Oxygen	25·67	25·19	

Taking the mean of the first two of the above weights, 5139, 5127·6, which also agreed nearly with the number 5100·3 obtained by Robiquet from hydrochlorate of narcotine crystallized from alcohol, M. Regnault constructed the formula $C_{44} H_{23} N O_{13}$.

* Communicated by the Chemical Society; having been read May 20, 1844.

† *Annalen der Pharmacie*, band vi. p. 35.

‡ *Ann. de Chim. et de Phys.*, t. lxviii. p. 139.

	Calculated.
$C_{44} = 3363\cdot4$	$65\cdot60$
$H_{23} = 287$	$5\cdot60$
$N \phantom{_{13}} = 177$	$3\cdot45$
$O_{13} = 1300$	$25\cdot35$
$\overline{5127\cdot4}$	$\overline{100\cdot00}$

More lately M. Liebig has examined the atomic weight and constitution of many of the organic alkalies by a new method proposed by himself[*]. These bases in most cases form insoluble double salts with bichloride of platinum, which in their constitution are exactly analogous to the bichloride of platinum and ammonium ; one equivalent of bichloride of platinum being united to one equivalent of the hydrochlorate of the base.   From the quantity of metallic platinum left after combustion, the atomic weight can be easily calculated.

The double salt of platinum and narcotine gave in 100 parts 14·508 and 14·64 of metallic platinum.   The atomic weights corresponding to these are 5930 and 5780.   The formula calculated by M. Liebig from these numbers with the analysis formerly made, was $C_{48} H_{24} N O_{15}$.

The following table exhibits the great difference in the composition of narcotine admitted by M. Liebig, Regnault and Pelletier, although the results of the analyses are nearly similar.

Atomic weight found.

	Liebig.		Regnault.	Robiquet.
	Old.	New.		
	4799	5930	5139	5100·3
		5780	5127·6	
			5253	

Composition in 100 parts.

	Liebig.	Regnault.		Pelletier.
Carbon .	65·00	64·91	65·30	65·16
Hydrogen	5·50	5·96	5·99	5·45
Nitrogen .	2·51	3·46	3·52	4·31
Oxygen .	26·99	25·67	25·25	25·08

Formulæ deduced.

	Liebig.		Regnault.	Pelletier.
	Old.	New.		
	C_{40}	C_{48}	C_{44}	C_{34}
	H_{20}	H_{24}	H_{23}	H_{17}
	N	N	N	N
	O_{12}	O_{15}	O_{13}	O_{10}

[*] *Annalen der Pharmacie*, band xxvi. s. 45.

These widely-differing formulæ, calculated from analyses agreeing so nearly, made it particularly necessary to determine anew the atomic weight. I undertook the subject at the request of Professor Liebig, to whose valuable assistance in the further prosecution of my investigation I consider myself much indebted.

A. *Composition of Narcotine.*

The determination of the platinum in the double salt of that metal was the method adopted in my investigations. The first attempts gave constantly varying results; the per-centage of platinum found being 15·441, 16·7, 14·303, 14·84, 16·1, 16·6. The following per-centages of platinum obtained by my friend Dr. A. W. Hofmann were kindly communicated to me, 16·63, 16·49, 16·77. From these numbers it appeared that the atomic weight of narcotine could not be deduced accurately from its platinum double salt. Upon carefully repeating the experiments, it was found that this difference arose from two sources of error. The first is, that the platinum double salt is decomposed if submitted to washing for too long a time. A portion of the double salt, already freed from the excess of the bichloride of platinum employed to precipitate it, was thrown on a filter and submitted to further washing. The fluid passed through of a pale greenish colour, from which, on adding a solution of hydrochlorate of narcotine, a precipitate was obtained.

I. 0·571 grm. of this precipitate, carefully dried, gave on combustion 0·09 metallic platinum = 15·76 per cent.

II. 0·996 of the salt still remaining on the filter gave platinum 0·1538 = 15·441 per cent.

The second source of error lay in the action of the bichloride of platinum under the influence of a temperature much below 100° C. For the purpose of causing the precipitate of the double salt to settle more quickly in an agglomerated form, it was placed in a water-bath and heated much below the boiling point. The salt speedily collected at the bottom of the vessel in a half melted state, and the supernatant fluid became of an orange colour, from which, on filtering and cooling, large dark red crystals mingled with long white needles were deposited. The same fact was observed on boiling a portion of platinchloride of narcotine in a larger quantity of water, with the view of getting it for analysis in a more distinctly crystalline form from its watery solution. On cooling the greater portion again precipitated. The mother-liquor, also of an orange colour, upon evaporation gave the same red crystals mingled with the white. From a portion of the former collected and burned,

the quantity of metallic platinum obtained per cent. was 22·45. The white crystals were found to be identical with opianic acid, already obtained by M. Liebig and Wöhler from nar- cotine, by the action of peroxide of manganese and sulphuric acid.

It is probable that in determining the atomic weight of narcotine from its platinum double salt, the error in most cases arose from the employment of too great an excess of the bichloride of platinum for precipitation, under the influence of heat. Bichloride of platinum under these circumstances acts just as any other oxidizing agent, being itself reduced to the state of protochloride. In those instances in which the per-centage of platinum is given high, it is likely that the salt analysed was mixed with a portion of these red crystals con- taining above 22 per cent. of platinum.

To avoid these two sources of error, pure narcotine was dissolved in dilute hydrochloric acid, and to the cold solution just so much bichloride of platinum was added as was suffi- cient for the precipitation. After removal of the fluid by the Bramah press, the dry mass was pounded, again thrown on a filter and washed with a small portion of distilled water. The salt thus prepared should be dried at a temperature below 100° C. In the following analyses different prepara- tions were employed.

I. 0·9905 grm. gave platinum 0·1585 = 16·00 per cent.; atomic weight calculated = 5135.

II. 0·9313 grm. gave platinum 0·1486 = 15·95 per cent.; atomic weight = 5159.

III. 0·907 grm. gave platinum 0·142 = 15·65 per cent.; atomic weight = 5304·67.

IV. 0·947 grm. gave platinum 0·149 = 15·73 per cent.; atomic weight = 5265·83.

A salt carefully prepared by Dr. A. W. Hofmann gave from

V. 0·7617 grm. 0·1208 of metallic platinum = 15·85 per cent.; atomic weight calculated from this = 5208.

VI. 0·6272 grm. gave platinum 0·0991 = 15·8 per cent.; atomic weight = 5232.

The atomic weight adopted from the mean of the last three analyses, which were most carefully made, is 5235. With this number I now attempted to construct a formula from my own analyses of the double platinum salt, and from those of narcotine itself made by M. Liebig, Pelletier and Regnault.

I. 0·6535 grm. platinchloride of narcotine, burned with chromate of lead, gave carbonic acid 1·0477, water 0·2455.

II. 0·4025 grm. gave carbonic acid 0·643, water 0·156.

These correspond in the hundred parts to

	I.	II.
Carbon	43·72	43·56
Hydrogen	4·17	4·30

From which the following formula is deduced :—

		Calculated.	Found.
C_{46}	= 3450	43·70	
H_{62}	= 325	4·10	
N	= 177		
O_{14}	= 1400		
Pt	= 1233·5	15·81	15·80
Cl_3	= 1327·9		

7913·4 atomic weight of double salt.

$2573·9 = HCl + PtCl_2$.
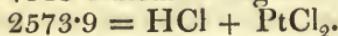
5339·5 = calculated atomic weight of narcotine.

Formula of narcotine taken from the above :

$$C_{46} H_{25} N O_{14}.$$
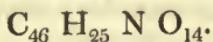
The same formula is deduced from the analyses of M. Liebig, Regnault, &c.

The atomic weight of carbon employed in this memoir being 75, gives in 100 parts for the analyses of these chemists—

	Liebig.	Regnault.		A. W. Hofmann.
Carbon .	64·09	64·01	64·50	64·53*
Hydrogen	5·50	5·96	5·97	6·21
Nitrogen .	2·51	3·46	3·52	3·30
Oxygen .	27·9	26·57	26·01	25·96
	100·00	100·00	100·00	100·00

Formula calculated from these numbers—

$$C_{46} H_{25} N O_{14}.$$
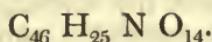
		Calculated.
C_{46}	= 3450	64·61
H_{25}	= 312·5	5·85
N	= 177	3·31
O_{14}	= 1400	26·23
	5339·5	100·00

B. *Products of Decomposition.*

I endeavoured by following out the products of decomposition produced by bichloride of platinum to see how far the analyses of these bodies would support the formula given above for narcotine.

* By this analysis the following numbers were found :—
0·4649 grm. narcotine gave $CO_2 = 1·100$, water = 0·26.
0·548 grm. do. gave 0·289 platinchloride ammonium.
0·4697 grm. do. gave 0·219 . . ditto.

An ounce and a half of narcotine, dissolved in dilute hydro-chloric acid, was precipitated by bichloride of platinum, and formed into a thin paste by the addition of distilled water. A large excess of bichloride of platinum, amounting fully to the quantity used for precipitation, was then added, and the whole heated in a basin over a fire. Upon the mass becoming blood-warm, the colour of the fluid portion changed from yellow to orange, and traces of opianic acid were formed. On further elevation of the temperature, but considerably before arriving at the boiling-point, the fluid assumed a still darker orange colour, the platinum double salt melted, and a quantity of opianic acid made its appearance. To ensure an uniform product ebullition was continued for half an hour, at the end of which time the opianic acid was re-dissolved and the surface of the liquid became covered with dark red crystals. These were separated by rapid filtration, and washed with hot distilled water to free them from all traces of the ac-companying acid. The acid was deposited in fine needles from the filtered liquid on cooling. The mother-liquor, de-canted and further evaporated, gave another portion mingled with long crystals of a rhombic prismatic form. These being more soluble than the opianic acid, were redissolved and sepa-rated by filtration.

The fluid evaporated to one-tenth its original quantity be-came of a very dark brown colour, gave off hydrochloric acid abundantly, and deposited large colourless crystals in the form of rhombohedrons, which upon resolution and rapid crystallization assumed their original figure of long rhombic prisms.

Whilst proceeding with my investigation, Prof. Liebig in-formed me that he had received a communication from Prof. Wöhler of Gottingen, stating that in studying the products of the decomposition of narcotine by the action of peroxide of manganese and sulphuric acid, on which he had for some time been engaged, he had already produced, among other results, some of the same compounds as those derived by me through the agency of the bichloride of platinum. I have therefore con-fined my investigation merely to the study of those substances I had already obtained, and have adopted the names proposed by Prof. Wöhler for those of them which had been also found by him.

The red platinum salt, platinchloride of cotarnin, crystal-lizes in large red, six-sided prisms, which are slightly soluble in hot water, and can be boiled in ammonia without suffering decomposition.

I. 2·679 grms. of this salt dried at 100° C. gave of metallic

platinum 0·614=22·91 per cent.: from which the calculated atomic weight is 2808·04.

II. 0·743 grm. of the same preparation gave platinum 0·17 =22·88 per cent.; atomic weight=2817·17.

III. 0·997 grm. of a different preparation gave platinum 0·23=23·06 per cent.; atomic weight=2773.

The combustion with chromate of lead gave the following numbers:—

1·1142 grm. dried at 100° C. gave carbonic acid 1·4055, water 0·334.

<div align="center">

Per cent.

Carbon   = 34·4

Hydrogen= 3·33

</div>

Admitting only one equivalent of nitrogen to be present, the formula deduced from these numbers is $C_{25} H_{13} NO_6$, HCl + Pt Cl_2, as is apparent from the results calculated and found.

	Calculated.	Found.
$C_{25}=1875$	34·7	34·4
$H_{14}=\ 175$	3·24	3·33
$N\ =\ 177$		
$O_6\ =\ 600$		
$Pt\ =1233·5$	22·89	22·88 22·91
$Cl_3\ =1328$		

      5388·5=atomic weight of the platinum salt.

Another analysis of this double salt, prepared directly from the base, will be given afterwards.

From the platinchloride of cotarnin the base can be easily separated by boiling in hydrosulphuret of ammonia. The method I pursued was the following:—The salt was first boiled in ammonia, in which it experienced no change, and while hot acted on by a stream of sulphuretted hydrogen; decomposition rapidly took place. The residue left, on carefully evaporating the mixture to dryness on the water-bath, was treated with distilled water, to which hydrochloric acid was added till the reaction was feebly acid, in order to decompose the soluble double salt of bisulphuret of platinum and ammonium formed during the reduction of the platinchloride of cotarnin. On filtering, a fluid passed through of a dark brown colour, from which, on the addition of caustic potash, the base was precipitated. The supernatant fluid, decanted off and evaporated till no trace of ammonia was left, gave another portion of base which had been held in solution by that alkali. The whole precipitate was again dissolved in dilute hydrochloric acid, boiled with animal charcoal, and after filtration again precipitated by potash.

Cotarnin when pure presents itself in the form of stellated

groups of colourless needles. It melts at 100° C. into a brown mass, without suffering decomposition, and loses about 7 per cent. of its weight. It is slightly soluble in cold, but more so in hot water. Alcohol forms with it a dark brown solution, from which the cotarnin cannot again be obtained in a crystalline form. In ammonia and æther it is readily dissolved, but is insoluble in potash. Strong nitric acid forms with it a dark red solution. The persalts of iron do not affect its colour. Its watery solution precipitates salts of copper and of the protoxide of iron.

The salts of cotarnin are obtained by dissolving this base in the dilute acids; they are extremely soluble.

0·1807 grm. of cotarnin, dried at 100° C. and burned with peroxide of copper, gave carbonic acid 0·437, water 0·1040.

$$\begin{array}{ll} & \text{Per cent.} \\ \text{Carbon} & =65\text{·}95 \\ \text{Hydrogen} = & 6\text{·}39 \end{array}$$

These numbers correspond to those calculated from the formula given by the platinum salt.

$$\begin{array}{lll} & & \text{Calculated.} \\ C_{25} = 1875 & & 66\text{·}61 \\ H_{13} = 162\text{·}5 & & 5\text{·}77 \\ N = 177 & & \\ O_6 = 600 & & \\ \hline 2814\text{·}5 = & \text{calculated atomic weight.} \end{array}$$

The difference between the carbon found and calculated arises partly from the small quantity of substance employed in the analysis, a second combustion was made of a larger portion of crystallized cotarnin dried over sulphuric acid.

0·0357 grm. of crystallized cotarnin dried over sulphuric acid gave carbonic acid 0·756, water 0·193.

0·5852 grm. of the same, for the determination of nitrogen, gave, according to the method of Will and Varrentrapp, 0·224 grm. of metallic platinum.

These numbers correspond to the formula $C_{25} H_{15} NO_8$.

$$\begin{array}{llll} & & \text{Calculated.} & \text{Found.} \\ C_{25} = 1875 & & 61\text{·}68 & 61\text{·}41 \\ H_{15} = 187\text{·}5 & & 6\text{·}16 & 6\text{·}38 \\ N = 177 & & 5\text{·}82 & 5\text{·}52 \\ O_8 = 800 & & 26\text{·}34 & 26\text{·}69 \\ \hline 3039\text{·}5 & & 100\text{·}00 & 100\text{·}00 \end{array}$$

The formula of crystallized cotarnin deduced from this is $C_{25} H_{13} N O_6 + 2Aq$, which is satisfactorily confirmed by a direct determination of the water of crystallization.

0·2463 grm. of crystallized cotarnin dried at 100° C, and completely melted lost 0·0185 grm. = 7·51 per cent.

0·2522 grm. lost 0·0182 grm. = 7·216 per cent.

	Calculated.	Found.
1 equiv. of anhydrous cotarnin = 2814·5		
2 equiv. of water . . , . = 225	7·44	7·51 7·216
1 equiv. of crystallized cotarnin = 3039·5		

Hydrochlorate of Cotarnin is formed by dissolving cotarnin in dilute hydrochloric acid. From this solution, by very long evaporation, the salt is obtained in long silky crystals, which are extremely soluble in water. With the bichlorides of mercury and gold it forms double salts, the colour of the latter being of a beautiful dark red.

I. 0·3215 grm. of this salt dried at 100° C. lost 0·049 grm. = 15·24 per cent.

II. 0·4577 grm. lost 0·0664 = 14·51 per cent.

III. 0·3907 grm. in beautiful crystals dried at 100° C. gave carbonic acid 0·8222 grm., water 0·203.

IV. 0·271 grm. gave water 0·139 grm.

These numbers give the following formula :—

$$C_{25} H_{13} N O_6, HCl + 5Aq.$$
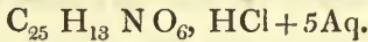
	Calculated.	Found.
$C_{25} = 1875$	57·36	57·39
$H_{14} = 175$	5·35	5·69 5·77
$N = 177·04$		
$O_6 = 600$		
$Cl = 442·65$		
3269·69		

	Calculated.	Found.
1 eq. of anhydrous hydroch. of cotarnin = 3269·69		
5 eq. of water . . . . = 562·5	14·67	14·51 15·24
1 eq. of crystallized hydroch. of cotarnin = 3832·19		

I again prepared the platinum double salt directly from the base : this on precipitation is of a pale yellow, but by drying assumes its characteristic dark red colour.

0·9418 grm. gave 0·2165 grm. of metallic platinum;

0·3873 grm. burned with chromate of lead gave carbonic acid 0·4968, water 0·1273.

These numbers correspond in 100 parts to—

	Calculated.	Found.
Carbon = 34·90		34·98
Hydrogen = 3·24		3·65
Platinum = 22·89		22·98

Opianic Acid.—This acid having been already examined

by Prof. Liebig and Wöhler, I did not enter into its investigation.

I. 0·5755 grm. gave on combustion with peroxide of copper carbonic acid 1·208, water = 0·250.

II. 0·504 grm. gave carbonic acid 1·0495, water = 0·223.

<table>
<tr><td></td><td colspan="2" align="center">In 100 parts.</td></tr>
<tr><td></td><td align="center">I.</td><td align="center">II.</td></tr>
<tr><td>Carbon</td><td>= 57·24</td><td>56·79</td></tr>
<tr><td>Hydrogen</td><td>= 4·82</td><td>4·91</td></tr>
<tr><td>Oxygen</td><td>= 37·94</td><td>38·30</td></tr>
<tr><td></td><td>100·00</td><td>100·00</td></tr>
</table>

If the atomic weight 2567 given by MM. Liebig and Wöhler[*] be taken as approximative, the formula of the crystallized acid will be—

<table>
<tr><td></td><td></td><td align="center">Calculated.</td></tr>
<tr><td>C_{20}</td><td>= 1500</td><td>57·41</td></tr>
<tr><td>H_9</td><td>= 112·5</td><td>4·36</td></tr>
<tr><td>O_{10}</td><td>= 1000</td><td>38·23</td></tr>
<tr><td></td><td>2612·5</td><td>100·00</td></tr>
</table>

Prof. Liebig kindly informed me that the formula established by M. Wöhler, who had fully studied this acid, agreed exactly with this.

Hemipinic Acid.—This acid accompanies opianic acid in all the processes of the oxidation of narcotine. After removal of the latter acid from the mother-liquor, the hemipinic acid is obtained by slow evaporation. The fluid becomes of a dark orange colour, almost approaching to black, and gives off hydrochloric acid abundantly. If the evaporation has been spontaneous, the hemipinic acid crystallizes in large flat rhombohedrons, which on resolution and rapid crystallization assume the form of long rhombic prisms.

The crystals of this acid effloresce on exposure to the air. It is more soluble in water than opianic acid, the solution having an acid reaction: alcohol and æther dissolve it readily.

From opianic acid it is distinguished by its forming insoluble salts with the oxides of lead, silver and peroxide of iron, all the salts of opianic acid being soluble. The hemipinate of the peroxide of iron is of a fine orange colour.

0·2505 grm. of the hemipinate of silver gave on combustion 0·1237 of metallic silver = 49·38 per cent., from which the calculated atomic weight is 1285·4.

0·402 grm. of the same salt gave 3·1975 of metallic silver = 49·12 per cent., corresponding atomic weight = 1299·5.

0·338 grm. of crystallized hemipinic acid dried at 100° C. gave carbonic acid 0·656, water 0·1395.

[*] *Traité de Chémie Organique,* par M. Liebig, t. ii. p. 656.

The corresponding formula is—

		Calculated.	Found.
$C_{10} =$	750	53·09	52·93
$H_5 =$	62·5	4·42	4·58
$O_6 =$	600	42·49	42·49
	1412·5	100·00	100·00

An unfortunate accident deprived me of the means of giving the combustion analysis of a pure silver salt. The combustion of an impure specimen gave the following approximative results :—

0·4985 grm. of substance gave carbonic acid 0·5115 grm., water 0·90 grm.

These numbers express—

		Calculated.	Found.
$C_{10} =$	750	27·30	27·98
$H_4 =$	50	1·81	2·00
$O_6 =$	600		
$Ag =$	1351·6	49·12	49·12
		100·00	100·00

The formula of crystallized hemipinic acid deduced from these analyses is $HO + C_{10} H_4 O_5$, and the atomic weight of the anhydrous acid 1300.

From its formula it appeared to be a product of the oxidation of opianic acid. To establish this point, a portion of the latter was boiled in an excess of bichloride of platinum. The liquid, separated by filtration from the undecomposed opianic acid, which is deposited on cooling, and submitted to further evaporation, gave crystals of hemipinic acid.

Narcogenin.—Besides those bodies already mentioned, the oxidation of narcotine also produces a base which is intermediate between it and cotarnin. Its platinum salt was accidentally obtained by me in endeavouring to prepare platinchloride of cotarnin. The exact circumstances under which it alone is produced has not been accurately ascertained. In my experiments it has generally been accompanied by a small portion of platinchloride of cotarnin. It appears however to depend upon the excess of bichloride of platinum, with which platinchloride of narcotine is boiled. On employing a large excess, not a trace of this base is to be found; the products being platinchloride of cotarnin, opianic acid, and hemipinic acid. If, on the other hand, a very small excess of bichloride of platinum is used, scarcely any cotarnin is formed; the products being the new platinum compound, opianic and hemipinic acid. This double salt, for which I propose the name of platinchloride of narcogenin, crystallizes in long needles of

a light orange colour, and is at once distinguished from the corresponding salt of cotarnin and narcotine by its colour and crystalline form, but more particularly by the action of ammonia. Upon the two latter salts this volatile alkali has no action, further than slightly darkening the platinchloride of narcotine, whilst platinchloride of narcogenin becomes paler on contact with it, and suffers complete decomposition by the application of heat.

I. 0·6455 grm. of platinchloride of narcogenin on combustion left metallic platinum 0·118 grm. = 18·03 per cent.; corresponding atomic weight of the platinum salt = 6841.

II. 0·6008 grm. gave platinum 0·1088 grm. = 18·10 per cent.; atomic weight = 6814.

III. 1·1735 grm. gave platinum 0·2105 grm. = 17·93 per cent.; atomic weight = 6879.

IV. 0·5803 grm. of another salt, which had been dissolved in boiling water and crystallized, gave platinum 0·1076 grm. = 18·54 per cent.; corresponding atomic weight = 6653.

The salts employed in the determination of these atomic weights were of different preparation. The following analyses are given rather as approximate than as accurate, on account of the difficulty of obtaining platinchloride of narcogenin quite free from other matters.

On combustion with chromate of lead,—

    I. 0·390 grm. gave carbonic acid 0·584, water 0·140
   II. 0·2665   ...   ...   0·3955, ... 0·1002
  III. 0·286   ...   ...   0·4255, ... 0·109

In 100 parts.

	I.	II.	III.
Carbon	40·83	40·47	40·57
Hydrogen	3·98	4·12	4·23

These numbers correspond to the following formula:—

	Calculated.	Found.
$C_{36} = 2700$	40·38	40·62 mean.
$H_{20} = 250$	3·73	4·11 ...
$N = 177$		
$O_{10} = 1000$		
$Pt = 1233$	18·44	18·15 ...
$Cl_3 = 1328$		
$\overline{6688}$		

Rational formula, $C_{36} H_{19} N O_{10} + HCl + PtCl_2$.

To separate narcogenin from its platinum compound, I intended to pursue the same plan as for the decomposition of the corresponding cotarnin salt. Complete decomposition,

however, took place on ebullition in ammonia. The precipitate, separated by filtration from its dark mother-liquor and washed with distilled water, was dissolved in weak hydrochloric acid, and after boiling with animal charcoal, was again filtered and precipitated by potash. The base thus obtained was in the form of a white, extremely hygroscopic powder.

0·2145 grm. of this powder, dried at 100° C., gave carbonic acid 0·506, water = 0·136.

	In 100 parts.	
	Found.	Calculated.
Carbon =	64·33	65·62
Hydrogen =	7·00	5·77

As the difference between the carbon and hydrogen found and calculated amounted to more than one per cent., I immediately prepared a platinum double salt directly from the base, to compare its analysis with that of the original platinum compound.

0·9255 grm. of this salt gave platinum 0·1455 = 15·73 platinum per cent.

The platinum per cent. in the original salt amounted to 18·10 grms.

0·3213 grm. burned with chromate of lead gave carbonic acid 0·5195, water 0·12.

	In 100 parts.	
	Found.	Calculated.
Carbon =	44·09	40·38
Hydrogen =	4·15	3·73

The two platinum salts have therefore a totally different constitution.

If an atomic weight of the base be calculated from 15·73, the platinum per cent. derived from the second platinum salt, the number obtained is 7846, from which, with the carbon and hydrogen found in the same platinum salt, the following formula is deduced, $C_{46} H_{25} N O_{14}$, $HCL + PtCl_2$.

		Calculated.	Found.
C_{46}	. .	43·70	44·09
H_{26}	. .	4·10	4·15
N			
O_{14}			
Pt	. .	15·81	15·73
Cl_3			

This formula, however, expresses the composition of platinchloride of narcotine.

The same resemblance in composition is shown by a comparison of the per-centage of carbon of the base separated from the platinchloride of narcogenin and of that of narcotine.

The hydrogen is a little too high, arising from the small quan-
tity of substance used in the analysis, and from the extreme
hygroscopic nature of the base in its precipitated state of pow-
der. A repetition of its analysis after crystallization from
alcohol gave 5·62 of hydrogen per cent.

	Base separated from platinchloride of narcogenin.		Narcotine.
Carbon	= 64·33		64·61
Hydrogen =	7·00	5·62	5·85

On crystallizing from alcohol, this base presents itself in the
same form as narcotine, with which in its other physical proper-
ties it is identical. The peculiar metamorphosis which narco-
genin had suffered on separation from its platinum double salt
made it necessary to investigate the fluid from which the changed
base had been separated. This ammoniacal liquid, which on
filtration possessed a dark red colour, deposited on cooling a
dirty whitish precipitate, from which it was decanted and then
acted upon by a stream of sulphuretted hydrogen. On pro-
ceeding in the same way as that adopted for separating cotar-
nin from its platinum compound, a light greenish-coloured
fluid was obtained, from which, on the addition of potash,
crystals having the form of cotarnin were precipitated. These,
after washing with distilled water, were redissolved in dilute
hydrochloric acid and precipitated by bichloride of platinum.
From this double salt, after combustion, the quantity of pla-
tinum per cent. obtained was 22·42. Platinchloride of cotarnin
gave 22·88. The agreement of the physical properties as well
as the amount of platinum per cent. prove the identity of the
two substances. These analyses of the two bases derived from
the decomposition of platinchloride of narcogenin, show that
the latter cannot exist in an isolated form, but on separation
from its double salt is converted into narcotine and cotarnin.
The former remains on the filter, while the latter passes through
in solution in ammonia, accompanied by the double compound
of platinum and ammonium, which has been reduced to the
state of protochloride by the decomposition of water. The
dirty looking precipitate which settles on the cooling of the
fluid, is perhaps a small portion of the salt of Magnus mingled
with foreign matter. The following table shows the decom-
position which takes place :—

2 equiv. of narcogenin . . . . . $= C_{72} H_{38} N_2 O_{20}$

2 equiv. of oxygen from water de-
composed in converting 2 equiv. of $\Big\} =$ O_2
Pt Cl$_2$ to the state of PtCl . . .

$$\overline{C_{72} H_{38} N_2 O_{22}}$$

Produce.

1 equiv. of narcotine $\quad . \quad . \quad . \quad . \quad . \quad = C_{46} \; H_{25} \; N \; O_{14}$
1 equiv. of cotarnin $\; . \quad . \quad . \quad . \quad . \quad . \quad = C_{25} \; H_{13} \; N \; O_{6}$
1 equiv. of carbonic acid $\; . \quad . \quad . \quad . \quad = C \qquad\qquad\quad O_{2}$

$$\overline{C_{72} \; H_{38} \; N_{2} \; O_{22}}$$
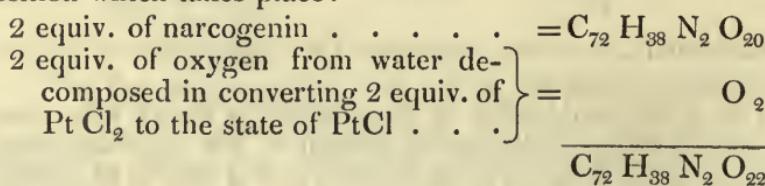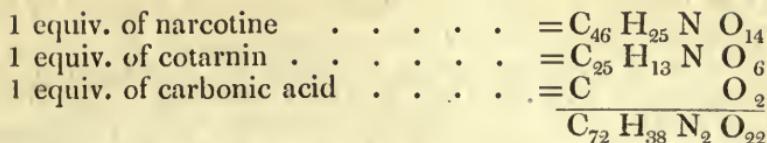
A comparison of the formula of narcotine deduced from direct analysis, and of its products of decomposition, show a very simple relation.

By the employment of a large excess of bichloride of platinum, the products obtained are cotarnin, opianic acid, carbonic acid and water. Along with these also is found the almost constantly accompanying hemipinic acid, derived from the further oxidation of the opianic.

1 equiv. of narcotine $. \quad . \quad . \quad = C_{46} \; H_{25} \; N \; O_{14}$
+7 equiv. of oxygen $\; . \quad . \quad . \quad = \qquad\qquad\qquad O_{7}$

$$\overline{C_{46} \; H_{25} \; N \; O_{21}}$$

Produce.

1 equiv. of cotarnin $. \quad . \quad . \quad = C_{25} \; H_{13} \; N \; O_{6}$
1 equiv. of opianic acid $. \quad . \quad = C_{20} \; H_{9} \qquad\; O_{10}$
1 equiv. of carbonic acid $\quad . \quad = C \qquad\qquad O_{2}$
3 equiv. of water $. \quad . \quad . \quad . \quad = \qquad H_{3} \qquad O_{3}$

$$\overline{C_{46} \; H_{25} \; N \; O_{21}}$$
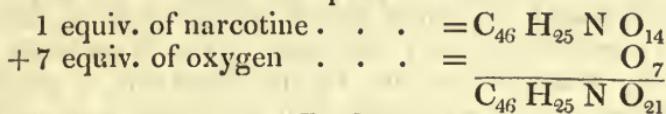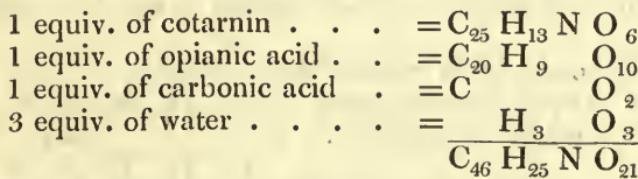
The further oxidation of opianic acid gives from

1 equiv. of anhydrous opianic acid $\quad . \quad = C_{20} \; H_{8} \; O_{9}$
+1 equiv. of oxygen $\quad . \quad . \quad . \quad . \quad . \quad = \qquad\qquad O$
2 equiv. of anhydrous hemipinic acid $\overline{= C_{20} \; H_{8} \; O_{10}}$
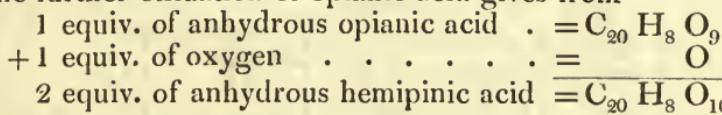
To prove the disengagement of carbonic acid, the oxidation by bichloride of platinum was conducted in an atmosphere of hydrogen. The amount obtained, though quite conclusive of the fact of its evolution, was not large, being according to theory only one equivalent in 46.

If only a very small excess of bichloride of platinum is employed, the decomposition of narcotine does not proceed so far. The principal product is now platinchloride of narcogenin, accompanied by smaller quantities of opianic and hemipinic acid. Traces of platinchloride of cotarnin are also observed.

2 equiv. of narcotine $\quad . \quad . \quad = C_{92} \; H_{50} \; N_{2} \; O_{28}$
+5 equiv. of oxygen $\; . \quad . \quad . \quad = \qquad\qquad\qquad O_{5}$

$$\overline{C_{92} \; H_{50} \; N_{2} \; O_{33}}$$

Produce.

2 equiv. of narcogenin $. \quad . \quad = C_{72} \; H_{38} \; N_{2} \; O_{20}$
1 equiv. of opianic acid $\quad . \quad = C_{20} \; H_{9} \qquad\; O_{10}$
3 equiv. of water $\quad . \quad . \quad = \qquad H_{3} \qquad O_{3}$

$$\overline{C_{92} \; H_{50} \; N_{2} \; O_{33}}$$
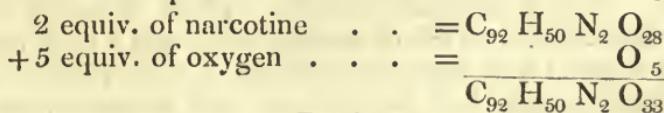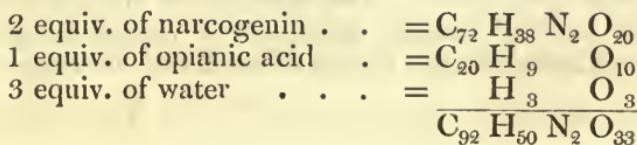
The hemipinic acid is, as stated before, a product of the decomposition of opianic acid.

The same precaution of conducting the oxidation in an atmosphere of hydrogen was also here observed. The lime-water became only very slightly opalescent after continuing the ebullition for a considerable time. The evolution of carbonic acid at this stage may be owing to the small quantity of cotarnin accompanying in most cases the formation of narcogenin; but more probably to the decomposition of the latter, which by further oxidation is converted into opianic or hemipinic acid, with disengagement of carbonic acid. Of the decomposition of platinchloride of narcogenin in this manner I convinced myself by direct experiment. A portion of this salt boiled in a large excess of bichloride of platinum with the precaution already mentioned, gave off carbonic acid freely, and was changed into the platinum salt of cotarnin. The mother-liquor separated from the latter gave crystals of opianic or hemipinic acid, according to the duration of the boiling.

The following table exhibits these changes :—

$$\begin{array}{ll} \text{2 equiv. of narcogenin} & = C_{72} \ H_{38} \ N_2 \ O_{20} \\ +\ 9 \text{ equiv. of oxygen} \quad . \quad = & O_9 \\ \hline & C_{72} \ H_{38} \ N_2 \ O_{29} \end{array}$$

Produce.

$$\begin{array}{ll} \text{2 equiv. of cotarnin} \quad . \ = C_{50} \ H_{26} \ N_2 \ O_{12} \\ \text{1 equiv. of opianic acid} = C_{20} \ H_9 \quad\ \ O_{10} \\ \text{2 equiv. of carbonic acid} = C_2 \quad\quad\quad O_4 \\ \text{3 equiv. of water} \quad . \quad . \ = \quad\quad H_3 \quad\ \ O_3 \\ \hline \quad\quad\quad\quad\quad\quad\quad\quad\quad C_{72} \ H_{38} \ N_2 \ O_{29} \end{array}$$
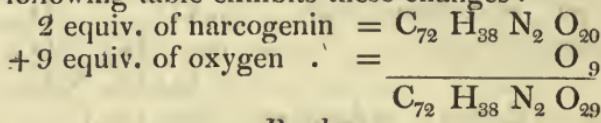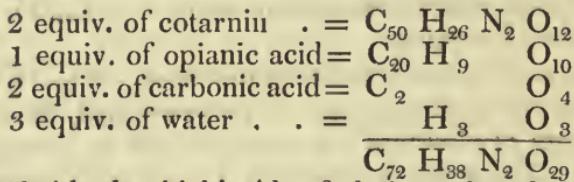
It is probable that bichloride of platinum has the same oxidizing action on many of the other organic bases. This circumstance may account for the difference of their atomic weights when calculated from their platinum double salts. Upon morphia its action is even more marked than upon narcotine. On boiling the former in an excess of the bichloride, the liquid becomes immediately of a very dark colour, apparently black, by reflected light. There is at the same time formed a platinum salt of another base, and a dark brown granular looking acid, which is insoluble in water, alcohol and æther, forms soluble salts with potash and ammonia, and an insoluble salt with silver. This subject has not yet been examined by me, but I intend to return to it, and hope before long to communicate the results of its further investigation.

LXV. *Observations upon the Decomposition of the Double Cyanides by an Electric Current.* *By* Mr. James Napier*.

FOR some time past I have considered that there were phæ-nomena presenting themselves in my daily operations of depositing metals by a galvanic current of sufficient scientific interest to warrant their being collected and given in detail to this Society. In anticipation of doing so, I carefully noted down what seemed most interesting, in hopes of being pre-pared to give them as a short series of papers next session : I say series, because so far as I have observed, all the double cyanides, even when they have the same constitution, do not comport themselves in the same manner, giving rise to differ-ent results, all of which could not be given in the compass of one paper. But these anticipations have been somewhat changed by the circumstance, that a paper upon a similar subject has lately been read by Prof. Daniell to the Royal Society. The facts brought forward in that paper are as yet unknown to me, further than that certain metals are not trans-ferred from one pole of the galvanic circuit to the other by means of the current ; this is one of the facts to which my at-tention has been directed for a long time, and I am induced to bring this paper forward to-night, not with any desire of sharing the honours due to the discovery, but in hopes that, while noticing this in connection with other facts which I hope to have the honour of laying before you next session, I may have a little more claim upon your attention, from its having been original on my part.

The present paper is more particularly devoted to cyanide of potassium and silver,—a double salt of general use in elec-tro-metallurgical operations, and one of a class eminently fitted to exhibit some of the most interesting facts in connection with electrical decompositions. It is known to those engaged in the deposition of silver from this salt, that if it be used pure, or in a neutral state, with a positive electrode of silver, no deposition of metal is obtained unless a battery of great power be used ; but if a little cyanide of potassium be added to the solution, a very weak current of electricity is sufficient to give a deposit. The usual explanation of these facts is, that pure or neutral cyanide of potassium and silver is a bad conductor of electricity, and that the addition of free cyanide of potas-sium gives it conducting power, either as being the conduct-ing medium, or by a kind of disposing influence which it im-parts to the salt decomposed. That one fluid imparts such an

influence to another I need hardly say is absurd; and it is well known that the conducting medium in any fluid is the substance decomposed. It is certain that the free cyanide of potassium, which is added with the idea of giving conduction, is not decomposed. The question then occurs, what part does it take in these operations? I have observed that, whilst endeavouring to deposit from the neutral salt, the positive electrode, although brightly polished previous to being put into the solution, instantly assumed a chalky aspect. It appeared probable that this was owing to the formation of cyanide of silver, which, being insoluble in the neutral salt, obstructed the current of electricity by its coating. To prove this, I placed a quantity of crystallized cyanide of potassium and silver, dissolved in water, in a long shallow vessel, kept the two electrodes at a considerable distance apart, and connected them with a battery of 20 pairs of plates for forty-eight hours: there was a considerable deposit of silver upon the cathode, the solution around it was strongly alkaline, and the precipitate formed by adding a drop of nitrate of silver was easily redissolved; whilst that part of the solution round the anode remained neutral, the anode was covered over with a grayish-white powder, which being washed exhibited every character of cyanide of silver. The use of the addition of free cyanide of potassium to the silver solution was now evident, not to facilitate the passage of the current, but to dissolve the cyanide of silver formed upon the electrode. To make this the more certain, I dissolved 2 equivalents of the cyanide of potassium, or twice 200 grains, and put them into a vessel divided by a porous diaphragm: into the division in which the anode was placed I put an equivalent of cyanide of potassium, or 66 grains; the electrodes being previously weighed, were connected with a battery of 4 pairs, deposition immediately took place; the current was continued for twelve hours; towards the end of the operation hydrogen gas began to be evolved from the cathode, and a powder to appear upon the anode; the experiment was then stopped; the anode being washed and weighed indicated a loss of 108·7 grains; the solution in which it was placed was neutral, and on being evaporated and fused yielded 214·3 grains of silver; the cathode had gained 107·4 grains in weight; the solution in which it was placed had a powerful alkaline reaction, smelled strongly of hydrocyanic acid, and on being tested scarcely indicated the presence of silver. This last circumstance attracted particular attention, and induced me to repeat the experiment under various modifications, also with gold, copper and zinc, all of which gave similar results.

Another question suggested itself in connection with the decomposition of cyanide of potassium and silver, namely, does the cyanide of potassium which is in union with the cyanide of silver take any part in these decompositions? To determine this, the experiment just detailed was repeated, taking care to reduce the power of the current of electricity when any indication of gas was perceived at the cathode; when the experiment was completed, the solution containing the negative electrode was tested for the quantity of cyanide of potassium by nitrate of silver; it indicated 67·4 grains, which is more than an equivalent, independent of loss by decomposition. The reason of the cyanide of potassium being more than the equivalent, is from that invariable endosmose which takes place when a current of electricity is passing which is in the direction of the negative division. From this experiment it appears that the cyanide of potassium which is combined with the cyanide of silver undergoes no change by the electric current, if it be properly regulated to the condition of the solution, so long as cyanide of silver is present: this exhibits beautifully the different conducting power of the two salts, and so long as there is not more electricity than the best conductor can transmit, it will pass through it in preference to any other salt mixed with it, although such other may be also an excellent conductor. But when the current is more than the best conductor will transmit, it passes through the next best present, which accordingly suffers decomposition; so that in the case of cyanide of potassium and silver, when the current is more than the cyanide of silver will transmit, the cyanide of potassium is also decomposed, so that two proportionals of cyanogen pass to the positive electrode, which being dissolved increases the quantity of silver in the solution, while potassium is liberated at the negative electrode, decomposing water with the escape of hydrogen gas.

If platinum electrodes be used there is no necessity for adding free cyanide of potassium, the pure solution being decomposed by a weak current, and the cyanogen liberated at the anode is absorbed by the solution, turning it dark brown and leaving a slight blackish precipitate, probably paracyanogen; this has been already observed by Professor Faraday in cyanide of potassium alone; but if the current be increased to 7 or 8 pairs of plates, oxygen gas is evolved from the anode, and cyanide of silver is precipitated upon its surface, the cyanogen being decomposed forming ammonia and probably formic acid, which latter decomposes the cyanide of potassium combined with the cyanide of silver, forming hydrocyanic acid and formiate of potash, the cyanide of silver being precipitated. Several other reactions take place, but my experiments

are not yet sufficiently numerous to allow of my venturing upon a description of them in this paper.

It is evident, therefore, that the cyanide of potassium and silver in a neutral state is a most excellent conductor of electricity. A little cyanide of potassium added does however facilitate its decomposition; and it is remarkable that when the cyanide of potassium added is in such proportion as to form a compound having two equivalents of cyanide of potassium to one of cyanide of silver, it is probably the most easily decomposed substance that we have. I have deposited silver from it easily with one square inch of copper and zinc immersed in water, the solution being at a temperature of 75°.

Another question suggested itself in connection with this salt, namely, the manner in which the cyanogen is transferred from the negative to the positive electrode, whether it travels direct, or is transmitted from particle to particle. Probably this has been answered satisfactorily long ago by other experimenters; but this being a salt eminently fitted to determine this question, I made the following experiment:—Into one division of the decomposition cell, a solution of pure cyanide of potassium and silver was introduced, and into the other division a neutral solution of chloride of potassium. By this arrangement, I considered that, if the cyanogen travelled direct, cyanide of silver should be formed upon the positive electrode which was immersed in the chloride of potassium; and if it was transmitted from particle to particle, chloride of silver would be deposited upon the pole and cyanide of potassium formed in the solution. The experiment verified this last supposition; after a current had passed through the solution for nearly eight hours the positive electrode was completely encased in a horny crust of considerable flexibility, which was not soluble in strong boiling nitric acid nor in hot sulphuric acid, and did not give off fumes of hydrocyanic acid on the addition of muriatic acid. 25 grains gave 18·6 of silver, which with the previous experiments is sufficient to prove the substance to be chloride of silver; the solution had the smell of cyanide of potassium, which had dissolved or decomposed a portion of the chloride of silver it held in solution.

LXVI. *On Sonorous Phænomena in Electro-Magnets.*
By J. P. Marrian.

To the Editors of the Philosophical Magazine and Journal.
Gentlemen,

HAVING noticed a few phænomena resulting from magnetic influence in bars of soft iron, and which I have not before heard of, I am induced to submit an account of

some experiments in the hope that they may lead to further investigation, as I am much inclined to think that many ex-traordinary facts may be gathered as well as important results obtained from a more extended prosecution of the subject. I have already made a great number of experiments, and shall proceed to detail a few of those which I consider to be the most interesting.

Having some considerable time since commenced a series of experiments on the conducting powers of different metals, among others I made use of several bars of soft iron, and found that when a bar of that metal is so circumstanced that it may be converted into an electro-magnet, at the instant that the magnetism is imparted *a sound is given from it,* and another sound is again perceptible when the galvanic current is broken; the two sounds however are not exactly alike, but appear to correspond with such as may be produced by the alternate separation and attraction of *the* particles or *certain* particles composing the material acted upon. In order how-ever that the subject may be more conveniently detailed, I will describe a few experiments.

I constructed a helix of thick copper wire, on the outside of which is another of a much thinner wire, with such an arrange-ment that they can be easily separated. I inserted a bar of soft iron one inch in diameter and twenty feet long in the com-pound helix, the primary wire of which I connected with a battery; on breaking the circuit a very audible sound was given from the iron, and again on forming contact. I then took bars of different dimensions, and varying in thickness from a quarter of an inch to two inches, and in length from six inches to twenty feet, and a sound was given by each, va-rying however in intensity; first, according to the size of the bar; second, to its diameter, filling or not filling the helix; third, the power of the battery; fourth, its position in the helix; for if the end were not placed beyond the centre of the primary coil no magnetic effects were produced, *and the bar did not sound*; and lastly, much appeared to depend on the state of the *secondary coil,* as the sounds diminished consider-ably on the ends being united, and the diminution was greater when the contact was metallic, than when I held the wires in my hands. The sound produced was the tonic of each bar, and I could imitate it by striking the end of the bar with any metallic substance, but could produce no resemblance by means of a blow laterally. I used electrodes of different metals, and tried numerous experiments in order to prove that the sonorous effects were not produced by the jarring of the ap-paratus; one of these was the partial envelopment of the

bar in wet clay and not suffering it to touch the helix, but the effect was still the same.   I tried bars of silver, copper, zinc, tin, lead and brass, but no similar effect was produced.

In three bars of equal sizes, one of soft iron, one of hard steel, and the third a permanent magnet, no sensible difference in these effects could be detected.

These experiments tend to prove that the particles of a bar of iron, when changing its electrical state, have a tendency decidedly *motive*, and that too along the bar in the direction of its axes.   I have not yet been able to prove that any excitation in the same direction will induce an electric current (which I think is highly probable), but have put together these facts in a hurried manner, in order that we may have an opportunity of hearing of such investigation as we may naturally expect from the immense amount of talent employed in this interesting branch of science.   I think also that these facts may serve to elucidate many others which I have not yet heard explained, but must reserve hypotheses for a future opportunity.

Yours, very respectfully,

Birmingham.   J. P. MARRIAN.

LXVII. *Lines of Inquiry connected with the Theory of Agriculture.   By* CHARLES DAUBENY, *M.D., F.R.S., Professor of Rural Œconomy in the University of Oxford*.*

I WILL now conclude by instancing a few lines of inquiry connected with the theory of agriculture, which nevertheless, from their intricacy, as well as from their having no direct bearing upon practice, do not seem of a nature to admit of being properly investigated, except it be upon an experimental farm.

The first of these relates to the power which plants possess of substituting one ingredient for another, as for example, soda for potass, lime for magnesia.

On this point we have at present some rather conflicting evidence.   Saussure, for example, tells us, that firs from two different soils contain different earthy constituents, although the sum total was as nearly as possible the same ; and Berthier has reported similar results as obtained by himself.   In these instances lime seems to have replaced potass, soda and magnesia, whilst in other cases an inorganic acid appears to have been substituted for an organic one, as the sulphuric acid in opium for the meconic.

* Extracted from the author's " Lecture on Institutions for the better Education of the Farming Classes, especially with reference to the proposed Agricultural College near Cirencester, &c.   Delivered at the Botanic Garden, Oxford, on Tuesday, May 14, 1844."   Oxford, 1844, 8vo.

Now it is highly important to ascertain the range within which this substitution can go on, because the result may serve to enlighten us as to the extent to which soda, and consequently common salt, are capable of supplying the want of potass—as to whether a soluble salt of lime will serve in the place of an alkali—and how far, in short, art has substitutes in store for the ingredients natural to a plant which may chance to be deficient.

Such an inquiry is the more important, because the views of physiologists on this subject strike one as not altogether consistent either with each other, or with themselves. Liebig for instance, in p. 66 *et seq.* of his Chemistry in its Application to Agriculture, seems to admit this power of substitution, and yet his whole theory of rotation, fallow, &c. appears to favour the contrary assumption. It is probable indeed that both principles are true within certain limits, and it therefore remains for future experimentalists to ascertain what these limits may be.

The inquiry may for convenience sake be distinguished as follows :—

1. How far have plants the power of substituting one fixed principle for another ?

2. How far will their growth be affected by the absence of one of the ingredients which they usually contain, provided the rest are supplied to them in a sufficient quantity ?

3. Are there any ingredients which, if present, are taken up by a plant, but the absence of which does not affect its growth or healthy condition ?

4. What becomes of a fixed ingredient which a plant does not assimilate ? Is it first absorbed and afterwards excreted, or is it rejected in the first instance by any power of selection which the secreting surfaces may possess ?

The next train of inquiries is suggested by the apparent inconsistency between the statements of Liebig in his last work, his Chemical Letters, and those of his former publication, on Chemistry in its Application to Agriculture. I say the *apparent* inconsistency, because I think a careful consideration of his remarks may lead us to infer, that what he intended to convey was merely the paramount importance of furnishing a plant with its inorganic constituents, to which the providing it with its organic principles was only subordinate. Possessed of the former, a plant will *in time* obtain from the atmosphere all the carbon and all the nitrogen which it requires ; *debarred* from the former, the most liberal supply of carbonic acid and of ammonia will be utterly ineffectual. But to infer from thence, that a plant, in Liebig's opinion, derives no advantage

from an abundant supply of those materials which afford it
these latter principles in greater abundance than nature her-
self could provide them, is to make that distinguished writer
contradict the very positions, which, more perhaps than any
others, arrested the attention of the public in his former
treatise, and contributed to the unexampled celebrity which it
enjoys.

Nevertheless, as we are dealing, not with the opinions of an
author, but with a question of fact, it would be interesting to
note the rate of growth in a plant, plentifully supplied with all
the fixed ingredients which it derives from the soil, but ob-
taining no supply of carbonic acid or of ammonia from any
other source than from the atmosphere, as compared with that
of another in which the fixed and volatile ingredients were
both furnished in equal abundance.   This inquiry might con-
sist—

1st. In ascertaining the relative growth of two plants, the
one manured with burned bones, or superphosphate of lime,
and with silicate of potass; the other also supplied with am-
moniacal salts.

2ndly. In ascertaining the effect upon the crops, of a given
weight of stable dung, from which the liquid part had been
carefully washed away, as compared with that of the same
amount where the latter had been carefully preserved.

Let us suppose for instance the dung from twenty cows to
be applied to a particular description of land, and that from
twice this number to be separated into its solid and liquid por-
tions, the solid portion being spread over a quantity equal to
the former, and the liquid portion over a field also of the same
dimensions and quality.   The results obtained from an expe-
riment so conducted might serve to enlighten us with respect
to the above question.

Another very important train of inquiries connected with
agriculture relates to the power which plants possess of de-
composing chemical compounds—the limits within which this
power is circumscribed—and the conditions under which it is
exercised.

We know, for example, that a plant can decompose a com-
pound whose ingredients are held together by a very close affi-
nity, namely, carbonic acid—we appear to possess an equal
certainty as to the decomposition of ammonia by the same
agency—we have reason to believe that certain neutral salts,
such as chloride of sodium, nitrate of soda, &c., obey the same
force—and we may suspect that the fertilizing influence of
other compounds is attributable to the same power—but we
still need some further facts to inform us what are the limits

of this faculty, and how far it may be dependent upon the stimulus of light.

Thus distinct experiments are requisite to determine, whether plants can decompose sulphate of ammonia, sulphate of lime, sulphate of magnesia, sulphate of soda, &c., as well as the chlorides of sodium, of magnesium, and of calcium; whether nitrates are converted into carbonates under the influence of vegetable processes, &c.

The power of deriving carbon from certain kinds of vegetable matter, as for example from *humus*, is attributed to plants by some, but denied to them by others; for although the latter thrive more in rich vegetable mould than they would do in any mere combination of earthy and alkaline ingredients, yet it is still disputed whether the mould does not assist vegetation, merely in proportion as it itself undergoes decomposition, and evolves in consequence carbonic acid.

Nor is this inquiry merely a speculative one, since if humus, *as such*, is capable of nourishing plants, everything which can contribute to render it soluble will augment its nutritive quality; whilst if it only acts by virtue of becoming decomposed, then our endeavours ought rather to be directed towards the acceleration of that decomposition, than towards the promotion of its solubility.

Another point upon which vegetable physiologists are divided is, whether plants are capable under any circumstances of absorbing nitrogen from the atmosphere; and although my own opinion inclines strongly to the negative side of this question, yet I am quite alive to the importance of having it set at rest by a more precise series of experiments than has ever yet been instituted. It would be needless indeed to remark, how much our practice would be modified by ascertaining that a plant is capable of furnishing itself with the nitrogen it requires. The whole theory of chemical manures would be changed by such a supposition, since, instead of setting them down, in so far at least as the volatile ingredients of plants are concerned, as the food, we should rather regard them in the light of stimulants to the vital forces of those organic bodies to which they are applied.

Another question, of much practical as well as theoretical moment, relates to the degree in which the adhesive or cohesive affinity, which subsists between the parts or particles of bodies, may act as a counteracting power to that of assimilation. How far, for instance, will the secretion of lime by a plant be favoured by the earth having already past through the system of an animal, as in the case of an oyster-shell, shelly marl, &c.? What difference will there be in the fertilizing

effect of phosphate of lime, when derived from bones, or from a rock possessing the same chemical constitution, but a different mechanical texture?

I am inclined to attribute to adhesive attraction more influence than is commonly ascribed to it, and believe that the same substance, which under ordinary circumstances is totally incapable of entering the vessels of a plant, may be absorbed by it when its particles are kept apart, and the force of their mutual affinity diminished, owing to the interposition of those of some foreign matter. The superior fertilizing agency of certain species of limestone to others, is perhaps dependent upon this cause.

It might therefore be interesting to manure the land with lime obtained from the harder descriptions of limestone and from the softer ones—from one sort destitute of organic matter, and from another composed of it; or with bones and with the phosphorite of Estremadura. The fertilizing influence of a comparatively small dressing of phosphate of lime treated with sulphuric acid seems to show, that in the ordinary condition in which bones are used as a manure, the cohesive attraction, which binds together their particles, is but slowly overcome by the decomposing agents to which they are subjected.

Another set of inquiries will relate to the theory of rotations and of fallows, subjects on which our present knowledge is purely empirical, and therefore imperfect and unstable.

It remains, for instance, to be seen, what may be the difference between the fertility of a soil exhausted by bearing a succession of the same or of different kinds of crops, without being in the mean time refreshed by manure. When the latter is subsequently applied to both, will the one on which various crops have been grown recover itself sooner than that which has received the same throughout? Should the latter point be established, it would lend some support to DeCandolle's theory of vegetable excretions, and a new subject of inquiry would then present itself, namely, how long do these excretions remain in the ground without becoming decomposed, and thereby losing their deleterious properties, and how far may their decomposition be accelerated by artificial means?

· The above may serve as examples of the kind of researches, which might be undertaken on an experimental farm, but which can hardly be expected to be elucidated in the ordinary routine of farming—others more especially relating to the soil may be pointed out, which come under the same category.

Thus it remains to be ascertained whether phosphoric acid is generally present in the soil—whether its quantity is pro-

portionate to that of the shells which the rock contains—how it can best be disengaged from its combination with the other ingredients of the rock, and be presented in a soluble form to the plant—whether it is generally associated with fluoric acid, in secondary rocks, as it is in primary ones—and whether the presence of the latter be favourable or injurious to vegetation.

I have studiously avoided particularizing any species of research which might have a direct practical tendency, being desirous of confining myself to those which could not well be pursued on a farm intended as a model for the neighbourhood, and a guide to the pupils educated at the establishment.

A multitude of other inquiries will however suggest themselves of a more practical character, such, for instance, as in a manure confessedly beneficial to a particular description of land, but containing various ingredients, to which of them the fertilizing influence is to be attributed. In the case of bones, for example, is it the animal matter, or the phosphate of lime, which improves the crop? in the case of the nitrates, is it the nitrogen of the acid, or the alkali which constitutes the base, that operates advantageously? Such inquiries, however, are too obvious to require to be particularly insisted on, but I must make one suggestion with respect to experiments instituted on manures, as of general application, namely, that it would be highly satisfactory, if in every trial the best proportion for use were ascertained, by manuring the field operated upon with quantities varying from the smallest to the largest amount which it has ever been recommended to administer.

Thus, suppose the quantity of guano added to a field of wheat or turneps to vary from 1 to 6 cwt., let the land be divided into six equal strips; let the first strip be drest with 1 cwt., the second with 2 cwt., and so on to the last, which will receive the largest amount, namely 6 cwt. By weighing the whole, or an aliquot part of the produce of each strip, the most advantageous proportion of this guano for land of the same quality may be estimated with something like precision, and a sort of standard laid down applicable to other similar cases.

The more subtle and delicate investigations which I have ventured to recommend may appear scarcely feasible in an infant institution, unsupported by royal patronage, and merely maintained by the voluntary contributions of individuals, who look to practical ends and to immediate results only.

That they would require a large expenditure of time and labour cannot indeed be denied, and that it might be deemed imprudent to divert the resources of the establishment to such uses, is not improbable; but if a fund were to be created for

this express object, it would seem far preferable to carry on such inquiries within the precincts of an agricultural college, than to frame for the purpose a distinct foundation, not only because many of the appliances needful for the undertaking would in the former case be already provided, but also because the mere inspection of such experiments would excite an interest amongst the pupils, and assist in stimulating their curiosity, and in familiarizing them with scientific researches.

The Royal Agricultural Society, which offers premiums for the determination of these and other similar questions, might, I conceive, with perfect consistency, supply the funds for their investigation; or, if such an expenditure were objected to, those members at least, who are sufficiently in advance of the general body, to be able to appreciate the advantage which agriculture would ultimately derive from inquiries of this nature, might naturally be expected to forward them.

It is a disgrace to the nation that so little of a public kind has as yet been accomplished, with reference to points on which the whole practice of husbandry turns, especially when, as in the instance of the determination of the constituents of the ashes of plants, the labour involved is such as places the research almost beyond the reach of an unassisted individual.

But as clearer views with respect to the principles of agriculture would render the utility of scientific knowledge to farmers more palpable, so on the other hand, increased knowledge on the part of the latter would create a stronger sense of the value of these apparently speculative inquiries, and thus the formation of an agricultural college would scarcely fail to give birth to experimental farms, as the existence of experimental farms would gradually pave the way to the foundation of agricultural colleges, even though at the commencement, that which came into operation first was carried on without the accompaniment of the other.

LXVIII. *Intelligence and Miscellaneous Articles.*

ON PARASELENÆ SEEN AT HIGH FIELD HOUSE, LENTON, NOTTINGHAMSHIRE. BY E. J. LOWE, ESQ.[*]

ON Wednesday, May 1, 1844, at 11^h 10^m p.m., a most remarkable and curious phænomenon made its appearance, on which the following are the remarks I made at High Field House, which is situated about $2\frac{1}{2}$ miles S.W. of Nottingham.

The day had been very fine and hot; the max. thermometer in shade 69°·30, in sun 91°; the barometer at 11 p.m. was rising, and was 30·722 inches; wind, slight breeze from E.

[*] Communicated by the Author.

At 11 p.m. I noticed the sky, and also five minutes afterwards, but nothing uncommon appeared. Few cirri of a very electric nature converged towards S., and a colourless lunar halo of about 28° in diameter and a lunar burr were visible.

At $11^h 10^m$ I again looked at the sky, which was cloudless, with the exception of a portion from S.E. to S.W., extending to the zenith, which had cirri dispersed thinly over it; and on a lower level were a few cumulostrati clouds, all converging towards S.; these clouds were very high. At this hour a most beautiful and rare phænomenon took place. A brilliant mock-moon, *a*, of a silvery colour, shone out of the lunar halo *b* on the W. side, and a little below the level of the moon. Also three arcs of inverted rainbows, *c*, *d* and *e*, appeared; the first, *c*, within the lunar halo *b*, and nearly at the highest part of it, which cut off upwards of 27° from the halo. The second, *d*, rested on the summit of the halo *b*, the circumference of the arc of which was about 28°, and the third, *e*, about 10° higher, the circumference of which was about 27°. All these arcs of inverted rainbows, or segments of circles, opened in a direction to the N.

Horizon. *Horizon.*

$11^h 13^m$.—At the points where the inverted rainbow *c* cut the halo *b* were two exceedingly bright oval lights or flames, evidently paraselenæ; and about 10° higher than the inverted rainbow *e* was a complete circle *h*, the diameter of which was nearly 8° (this extended to the zenith), having Cor Caroli nearly in the centre of the circle.

$11^h 15^m$.—The mock-moon *a*, and also inverted rainbow *c*, had vanished. The circles *b* and *h* and inverted rainbows *d* and *e*, together with the mock-moons *f* and *g*, still remained bright.

$11^h 20^m$.—This phænomenon had vanished, except the lunar halo and burr, which disappeared at half-past eleven o'clock.

[On the lunar halo *b*, opposite to the mock-moon *a*, some cumulostrati clouds rested, which, had there been a mock-moon there, would have been quite sufficient to have hidden it from our sight.] This phænomenon did not reappear.

No prismatic colours were exhibited during these very rare paraselenæ, which I think is usual with these remarkable appearances.

It may be as well to add, the weather for the last fortnight has been very fine and hot, and the sky nearly cloudless, but for the last few evenings the distant prospect has been remarkably clear.

High Field House, Lenton, Nottinghamshire, E. J. LOWE.
  May 6, 1844.

ANALYSIS OF THE PURPLE POWDER OF CASSIUS. BY M. FIGUIER.

In our last Number we gave the author's mode of preparing this compound; we shall now state his analysis of it.

The author remarks, that this substance has engaged the attention of numerous chemists, several of whom he names, and he considers that its composition is still not well understood; and he observes, that it has been regarded,—1st, as a mixture, or true combination of gold and stannic acid; 2nd, as a mixture of oxide of gold, or of metallic gold and peroxide of tin; 3rd, as a compound of protoxide of tin and hypothetical binoxide of gold, and afterwards as a compound of protoxide of gold and a peculiar oxide of tin, $St^3 O^3$; 4th, as a mixture of subdeutochloride of tin and metallic gold; 5th, as a stannate of an unknown oxide of gold; 6th, as a double stannate of binoxide of gold and of protoxide of tin; 7th, as a double stannate of protoxide of gold and protoxide of tin.

M. Figuier states, that from the results of his experiments he considers that the purple of Cassius is a perfectly definite compound of peroxide of tin and protoxide of gold; the clearest proof which can be given of the fact is, that the purple is formed in a direct manner by the admixture of stannic acid and protoxide of gold; to make this experiment, it is merely requisite to boil protoxide of gold in a solution of stannate of potash obtained by dissolving in potash stannic acid derived from the action of nitric acid on tin. The purple separates in the state of a flocky precipitate, different however, though but slightly, in its colour from the protoxide employed; analysis shows that the compound is similarly constituted to the purple of Cassius.

Other facts lead to the same conclusion. The purple of Cassius, treated with hydrochloric acid, yields a solution of bichloride of tin without any trace of protochloride; boiling potash also takes stannic acid from the purple powder, which may be proved by saturating the alkali; these facts prove then, that stannic acid exists in the purple.

The existence of protoxide of gold in this compound is unquestionable, for the purple of Cassius, and the protoxide of gold, when dried, have exactly the same colour; added to which, when hydrochloric acid is made to react on the purple, the residue of the action consists entirely of gold, which is what would be expected in this case, for the hydrochloric acid decomposes the protoxide of gold, forming water and leaving metallic gold.

Adopting M. Fremy's opinion, that three equivalents of stannic acid always enter into the composition of a neutral stannate, M. Figuier gives the following as the formula and composition of the purple powder of Cassius, the results arrived at being obtained by different methods:—

$$3(StO^2)A^2O + 4HO, \text{ or}$$
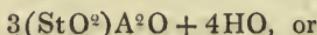
				By expt.
Three equivalents of stannic acid	2805 or 48·02	48·61
One ...	protoxide of gold	2586	44·30	44·41
Four ...	water	450	7·68	6·98
		5841	100·	100·

M. Figuier remarks, that this view of the constitution of the purple

powder is confirmed by the existence of a corresponding compound, which contains precisely twice as much stannic acid, and which of course is a bistannate of protoxide of gold.

This was prepared and analysed by Berzelius, and was found to consist of

			By expt
Six equivalents of peroxide of tin....	5610 or 68·45	68·46
One ... protoxide of gold..	2586	31·55	31·54
		100·	100·

Ann. de Ch. et de Phys., Juillet 1844.

ON FULMINATING GOLD. BY M. FIGUIER.

The author remarks, that there exist two different theories with respect to the constitution of fulminating gold ; Proust, Berthollet and some other chemists having considered it as a pure and simple combination of ammonia and oxide of gold. M. Dumas, on the other hand, in a work published in 1830, has stated the opinion that this substance is a compound of one equivalent of azoturet of gold and two equivalents of ammonia ; the azoturet of gold acting as an acid towards the ammonia.

M. Figuier is of opinion, that the theory of Proust and Berthollet is more in harmony with the facts of the case ; protoxide of gold yields, like the auric acid, a detonating compound with ammonia ; and we must then admit, adopting the opinion of M. Dumas, a new and corresponding azoturet of gold ; hitherto, however, no compound whatever of azote and gold has been obtained.

Added to this, the fulminating compounds of gold at present known possess exactly the colour of the oxides which furnished them ; the fulminating gold containing the protoxide, like the oxide itself, has a violet-blue colour ; the fulminating gold, obtained by putting ammonia in contact with teroxide of gold precipitated by nitric acid from the aurate of potash, is, like the oxide itself, of an olive colour ; lastly, that which is obtained by treating chloride of gold with ammonia is yellow, like the hydrated teroxide, separated cold by potash from a dilute solution of the chloride of gold, or like the same oxide precipitated by a weak acid from aurate of potash. The identity of colour of the oxides of gold, and their corresponding fulminating compounds, leads to the opinion that the oxide of gold enters in these compounds without altering its nature, and that they are all simple saline combinations of ammonia, in which the oxides act as acids.

M. Figuier insists principally on the fact, that the analyses of fulminating gold, containing auric acid, performed by M. Dumas, agree perfectly with this mode of regarding these compounds.

M. Dumas found the composition of fulminating gold prepared with auric acid to be $(Au^2Az) + AzH^3 + H^3O^3$; it is evident, however, that it may be stated more simply thus, $Au^2O^3 + 2(AzH^3)$, that is to say, as representing a sub-aurate of ammonia. Moreover fulminating gold, obtained with the chloride and ammonia, yielded M. Dumas by analysis,

Gold ....   74 in 100
Azote....   9·5 in 100

These numbers are represented by the formula $Au^2O^3 + 2(AzH^3)$ + HO, which represents the preceding compound as hydrated, and which requires

Gold ....   75 in 100
Azote ..   10 in 100

It can scarcely be doubted that this compound contains water.

To the above statements M. Figuier has made the following additions, which we translate at length :—

If to these considerations it be added, that the theory of M. Dumas was proposed at the period in which electro-chemical opinions were in highest favour, and that thus circumstanced he was guided especially by the analogy of the composition then attributed to the amalgam of ammonium (considered as a combination of azoturet of potassium and ammonia) ; and if it also be remarked, that analysis sufficiently agrees with this point of view, we shall probably be led to think that the ancient opinion of Proust and Berthollet is still the more probable one.

I am, however, continues M. Figuier, far from believing that these are the last words which will arise on this occasion.   On all occasions, when the true nature of a compound containing ammonia is discussed, too much attention and reserve cannot be exerted. Some chemists will be tempted to see combinations of amide in these various kinds of fulminating gold, others may discover compounds obtained by the substitution of oxide of gold for the hydrogen of the ammonia ; but I am of opinion that we ought not to be too much pre-occupied with these questions, which will be naturally resolved, with many others, when we possess more certain knowledge of the constitution of ammoniacal compounds.—*Ann. de Ch. et de Phys.*, Juillet 1844.

ON THE TEROXIDE OF GOLD.   BY M. FIGUIER.

Analysis of the Protoxide of Gold.—After trying various modes of analysis, such as reducing the oxide by hydrogen, and decomposition by hydrochloric acid, the author ascertained that the mere action of heat on this oxide was the most exact and ready method of analysis. Having determined by previous trial the accuracy of this process in analysing the tritoxide of gold, the author repeatedly found the number indicated by theory within the first place of decimals, and this, moreover, confirms the accuracy of the atomic weight of gold as determined by Berzelius.   The following is the simple method of analysis adopted :—an indeterminate quantity of the oxide, but amounting to nearly 15 grains, was put into a very narrow glass tube, one end of which was blown into a bulb.   Allowance was made by calculation for the small quantity of air contained in the tube, and as its capacity never exceeded about 1·5 cubic inch, it will be seen that the error resulting from the uncertainty of the temperature will entirely disappear on account of the smallness of the quantity of the air.   The

oxygen was received over water, and the metal weighed after calci-
nation.

Four analyses of the protoxide of gold dried *in vacuo*, and obtained
by diffcrent processes, were performed ; in the first two experi-
ments the protoxide was obtained with nitrate of mercury ; the third
from protoxide obtained by potash from the chloride ; the fourth
from tritoxide and potash.   In the last two cases the gold was treated
before weighing with hydrochloric acid ; for the oxide thus obtained
always retains a little alkaline sesquicarbonate, which is not removed
by washing.

The volume of oxygen obtained, reduced to weight, gave the fol-
lowing results as the composition of the protoxide of gold in 100
parts :—

Oxygen..	3·95	3·85	3·52	3·78
Gold....	96·05	96·15	96·48	96·22
	100·	100·	100·	100·

The mean of which gives

Oxygen..	3·77
Gold....	96·23
	100·

M. Figuier considers the protoxide of gold as composed of

One equivalent of oxygen ..	100
One equivalent of gold	2486
	2586

PROPERTIES OF OPIANIC ACID.

M. Wöhler states that opianic acid crystallizes in small slender
prisms, often as arborizations and usually reticulated.   It is colour-
less, though it is not always obtainable without a slight yellow tint ;
it is but slightly soluble in cold water, but very readily in hot, so that
a saturated boiling solution becomes almost one mass by cooling ; it
is also soluble in alcohol and in æther ; it fuses at 284° without
losing water ; it rises up the sides of the retort when heated in one,
and distils without being properly volatilized ; when heated in the
open air it smokes, and volatilizes with an aromatic odour, somewhat
resembling vanilla, and is very much like that which narcotina gives
out when decomposed.   The vapour is inflammable and burns with
a sooty flame.

Opianic acid which has been melted by heat remains soft and trans-
parent several hours after it has become cold, and may be drawn out
into threads like turpentine.   It afterwards begins to become opake
and hardens at the surface, so slowly, however, that in pieces of con-
siderable size a vitrified portion remains, as happens with arsenious
acid.   When examined in this state by the microscope, no well-cha-
racterized crystallization is perceptible in the opake hard portion ;
and it is very remarkable that the acid has become insoluble in
water, alcohol or dilute alkaline solutions.   If it be put into water
while it is transparent and be then heated, it becomes milk-white
in an instant ; on boiling it is converted into an earthy-white mass,
a very small portion of which is dissolved and deposits on cooling in

flocculi, which appear to be amorphous, but when examined by a strong magnifier they are seen to be composed of small four-sided and long fibrous crystals.   Opianic acid acts in the same way with alcohol ; and in this state it is insoluble in hot ammonia, and also in a solution of potash, unless long boiled in it.

It was found by analysis that the fused insoluble acid had the same composition as the soluble crystallized acid.   The composition is stated by the author to be

Carbon	57·84 or C^{20}
Hydrogen ..	4·29 ... H^8
Oxygen	37·87 ... O^9
	100·

combined with one equivalent of basic water.—*Ann. de Ch. et de Phys.*, Octobre 1844.

NATURE AND COMPOSITION OF SOME OPIANATES.

M. Wöhler states that a boiling saturated solution of opianic acid dissolves the carbonate of barytes, lime, lead and silver, with the disengagement of carbonic acid, and it forms soluble salts with these bases.

Opianate of barytes is very soluble and crystallizes in radiating prisms ; it effloresces when heated, and loses six per cent., or two equivalents of water.   Opianate of lead forms transparent, brilliant, mammillated crystals, which are but slightly soluble.   It contains 5·45 per cent. of water or 2 equivalents ; when heated, it first loses this water, then fuses and is eventually decomposed.   At a certain temperature the salt crystallizes free from water in small fasciculated silky crystals.   It is soluble in alcohol.

Opianate of silver crystallizes in short transparent crystals, which when seen in mass have always a yellowish tint, which appears to be produced by the action of light, but it does not subsequently increase ; it contains water, which it loses when heated and becomes opake ; it fuses when more strongly heated, and when decomposed it yields a liquid of a fine green colour and metallic lustre ; it afterwards assumes a deep red colour, and eventually leaves metallic silver ; these colours appear to depend on the formation of a salt of the suboxide.—*Ann. de Ch. et de Phys.*, Octobre 1844.

PREPARATION OF OPIANIC ACID.   BY M. WÖHLER.

To prepare this acid dissolve narcotina in an excess of dilute sulphuric acid, and add to the solution pure and very finely powdered binoxide of manganese, and boil the mixture.   The reaction, which takes place immediately, is shown by the liquor becoming of a yellow colour, and the evolution of a small quantity of carbonic acid gas ; the ebullition is to be continued as long as carbonic acid is evolved, adding occasionally binoxide of manganese, so that there shall be excess of it at the end of the operation ; and care must especially be taken that there is always an excess of acid.   The liquor is to be filtered while hot, and it is of a reddish-yellow colour ; on cooling it deposits a mass of opianic acid in small yellow crystals,

which are to be separated by filtering paper, washed with cold water, and then strongly pressed between folds of filtering paper. They are then to be dissolved and boiled in a solution of hypochlorite of soda to decolorize them, and the solution is to be decomposed by gradually adding hydrochloric acid. The opianic acid usually crystallizes in a colourless state on the cooling of the liquid; after washing it with cold water, the acid is to be again pressed and purified by repeated crystallizations.

The liquor from which the crystals are originally obtained is to be treated afresh with sulphuric acid and manganese to obtain more of the acid.—*Ann. de Ch. et de Phys.*, Octobre 1844.

ON SILICIC ÆTHERS. BY M. EBELMEN.

The author has procured two definite combinations of æther with silica, by means of the action of absolute alcohol on chloride of silicium; the circumstances under which they are produced are the following:—

Cautiously pour absolute alcohol into chloride of silicium, and a vivid reaction takes place with a very abundant disengagement of hydrochloric acid gas, and a considerable reduction of temperature; when the weight of the alcohol added rather exceeds that of the chloride of silicium, no further disengagement of gas is perceived, and the liquor becomes very sensibly warm. If it be submitted to distillation, there first passes over a certain quantity of hydrochloric æther, and then the greater part of the liquid contained in the retort distils between 320° and 340° F.; the latter product is to be set aside and the distillation is to be continued until it terminates at above 570° F.; there remains a mere trace of silica in the retort.

The product distilled between 320° and 338° was rectified until its boiling point became fixed between 323° and 325°; by this there was obtained a colourless liquid, possessing a penetrating and æthereal odour, a strong hot taste, and its density was 0·932. Water does not dissolve it, and decomposes it very slowly with a deposit of silica; it is perfectly neutral to test papers; alcohol and æther dissolve it in all proportions; the alcoholic solutions of the alkalies decompose it rapidly, and by means of acids, silica is separated in the gelatinous state. When a few drops are thrown upon red-hot platina, they burn with a white flame, depositing silica in the state of an impalpable powder.

The analyses of this compound show that it contains carbon and hydrogen in the same proportions as in æther, and that the silica contains the same quantity of oxygen as the base.

If then 277·32 be admitted as the equivalent of silicium, as allowed by most chemists, and SiO^3 for the formula of silica, that of the æther will be $SiO^3 \, 3C^4H^1O$. If, on the other hand, we take the third of the preceding number, or 92·44, as the equivalent of silicium, and SiO as the formula for silica, as proposed by M. Dumas, according to the density of the vapour of chloride of silicium, the formula for silicic æther will be similar to that of other compound æthers, and represented by $SiOC^4H^5O$.

The density of its vapour was found to be 7·18; calculation gives 7·234, admitting that $SiO C^4 H^5 O$ represents a volume of the vapour; this mode of condensation has not been hitherto observed in compound æthers.

On receiving in separate portions the product distilled between 338° and 570°, it was found by analysis that the carbon and hydrogen were constantly in the same proportions in the æther, but the proportion of silica increased with the temperature. The liquid distilled at above 570° is colourless, and has a weak smell and a taste different from that of the æther above described. Its density is 1·035. The action of water and of the alkalies upon this compound is perfectly similar to that upon the æther, $SiO C^4 H^5 O$; its analysis indicates its formula to be $(SiO)^2 C^4 H^5 O$.

Silicic acid forms then at least two æthers, and their formation is readily explained by the two following formulas:

$$SiCl + C^4 H^6 O^2 = HCl + SiO C^4 H^5 O,$$

$$(SiCl)^2 + 2 C^4 H^6 O^2 = C^2 H^5 Cl + HCl + HO + (SiO)^2 C^4 H^5 O.$$
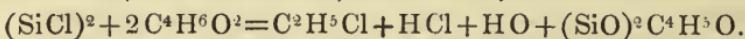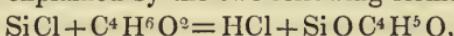
It is to be remarked that, during the reaction of alcohol on the chloride of silicium, hydrochloric acid is disengaged only as long as the chloride is excess; and it is only when the last portions of alcohol have been added that hydrochloric æther can be obtained. According to the formulas, 1 equivalent of alcohol, 575, must be employed for 1 equivalent of chloride of silicium, 535; and these were the proportions of these two bodies, which were, in point of fact, made to act upon each other.

The author has tried the action of alcohol upon the chlorides of tin, titanium, arsenic and phosphorus, the results of which he intends to lay before the Academy.—*Journ. de Pharm. et de Ch.*, Octobre 1844.

TRIGONOMETRICAL NOTATION.

Mr. Drach proposes the following:—

$\sin ^> x, \cos ^> x$, &c. for arc $(\sin = x) = \cos^{-1} (x - \frac{1}{2}\pi)$, &c.,

and $\sin^{-1} x$ being legitimately $1 \div \sin x$, and so forth.

CORRECTIONS IN THE TRANSLATION OF M. WARTMANN'S PAPER, IN OUR LAST NUMBER.

The best thanks of the Editors are due to their Glasgow correspondent who signs himself "A Subscriber," and who directs their attention to some oversights in the translation of M. Wartmann's paper, contained in the last number of the Philosophical Magazine.

In p. 269,

$$T = e q \left(\frac{a - M}{r}\right).$$

should have been printed thus,

$$T = e q^{\left(\frac{a-M}{r}\right)}.$$

P. 272, line 23, *for* "currents in contrary directions," *read* "currents in the same direction."

P. 273. For the rendering of paragraph 25 *a,* the following should be substituted :—

" When the length of one of the inductor wires remains invariable, and that of the other is gradually increased, for lengths of the additional wire increasing in geometrical progression, the intensities of the induced current measured by the rheometer diminish according to an arithmetical progression the first term of which corresponds to the sum of the effects of the inductor wires when the length of the additional wire is null, and the last term of which is equivalent to the action of the constant inductor wire taken isolatedly, or to such a length of the additional wire that the lengthened circuit be an infinitely less good conductor than the invariable circuit."

P. 274, for 26 *a, read* as follows :—

" For additional lengths of the inductor wire increasing in geometrical progression, the deviations of the rheometer measuring the difference of intensity of the two currents simultaneously induced increase according to an arithmetical progression, of which the first term is zero and the last equivalent to the action of the constant inductor wire, taken isolatedly, or to an infinite additional length."

METEOROLOGICAL OBSERVATIONS FOR SEPT. 1844.

Chiswick.—September 1. Slight fog : exceedingly clear, hot and dry. 2. Foggy : clear, hot and dry. 3. Fine : overcast. 4. Densely overcast and fine. 5. Cloudy and fine. 6. Rain : cloudy. 7. Slight rain : cloudy : clear and fine. 8. Very fine. 9. Densely overcast. 10. Overcast and fine. 11. Slight fog : very fine. 12. Overcast and fine. 13, 14. Very fine. 15. Cloudy : rain. 16. Very fine. 17. Showery : heavy rain at night. 18. Rain. 19. Clear and very fine. 20. Drizzly : hazy. 21. Very fine. 22, 23. Cloudy : very fine. 24. Very fine. 25. Slight fog : very fine. 26, 27. Mornings foggy : clear and fine. 28. Dense fog : heavy dew : clear and exceedingly fine. 29. Drizzly : cloudy and fine : clear : slight frost. 30. Slightly overcast : clear and fine.—Mean temperature of the month 1°·8 above the average.

Boston.—Sept. 1, 2. Fine. 3. Cloudy. 4. Windy. 5. Windy : lightning at night. 6. Cloudy : rain, with thunder and lightning from noon to 5 P.M. 7. Gloudy. 8. Fine. 9. Rain. 10, 11. Fine. 12—14. Cloudy. 15. Stormy : rain early A.M. 16. Fine. 17. Cloudy : rain early A.M. : rain A.M. 18. Cloudy. 19. Fine : rain early A.M. 20. Fine : rain early A.M. : rain P.M. 21, 22. Cloudy. 23. Windy : rain A.M. and P.M. 24. Fine : rain P.M. 25—28. Fine. 29. Cloudy : rain early A.M. 30. Cloudy.

Sandwick Manse, Orkney.—Sept. 1. Clear. 2. Clear : hot. 3. Clear : drizzle. 4. Fog : fine : fog. 5. Bright : clear. 6. Cloudy : fine : fog. 7. Damp : cloudy. 8. Cloudy. 9. Cloudy : showers. 10. Bright : clear. 11, 12. Bright : showers. 13. Bright : clear. 14. Frost : clear. 15. Damp : drizzle. 16. Damp : drizzle : showers. 17. Drops : drizzle : showers. 18. Frost : clear : cold. 19. Clear : aurora. 20. Frost : clear : aurora. 21. Frost : clear. 22, 23. Clear. 24. Clear : cloudy. 25. Drizzle : cloudy. 26. Showers : drizzle. 27. Damp : drizzle. 28. Cloudy : clear. 29. Showers : clear. 30. Rain.

Applegarth Manse, Dumfries-shire.—Sept. 1—4. Fair and fine. 5. Fair and fine, but cloudy. 6. Showery : lightning. 7. Shower : thunder. 8. Shower. 9. Fair and fine. 10. Fair and fine, but cloudy. 11. Fair and fine : one slight shower P.M. 12. Rain P.M. 13. Rain all day. 14. Rain very heavy. 15. Fair. 16. Shower. 17. Showery. 18—22. Fair : fine harvest day. 23. Fair, but cloudy. 24. Fair : clear. 25. Fair : cloudy. 26. Fair. 27. Fair : cloudy. 28. Rain all day. 29, 30. Fair and clear.

Meteorological Observations made by Mr. Thompson at the Garden of the Horticultural Society at CHISWICK, near London; by Mr. Veall, at BOSTON; by the Rev. W. Dunbar, at Applegarth Manse, DUMFRIES-SHIRE; and by the Rev. C. Clouston, at Sandwick Manse, ORKNEY.

Days of Month 1844. Sept.	Barometer Chiswick Max.	Chiswick Min.	Boston 8¾ a.m.	Dumfries-shire 9 a.m.	Dumfries-shire 9 p.m.	Orkney, Sandwick 9¼ a.m.	Orkney, Sandwick 8¼ p.m.	Thermometer Chiswick Max.	Chiswick Min.	Boston 8¾ a.m.	Dumfries-shire Max.	Dumfries-shire Min.	Orkney, Sandwick 9¼ a.m.	Orkney, Sandwick 8¼ p.m.	Wind Chiswick 1 p.m.	Boston	Dumfries-shire	Orkney, Sandwick	Rain Chiswick	Boston	Dumfries-shire	Orkney, Sandwick
1.	30·380	30·335	29·71	30·25	30·25	30·20	30·25	84	44	64	71½	45	64	56½	se.	calm	sw.	s.	……	……	……	……
2.	30·322	30·192	29·76	30·26	30·18	30·27	30·27	79	50	63·5	75½	46	68	60	e.	calm	nw.	se.	……	……	……	……
3.	30·097	30·020	29·62	30·16	30·15	30·27	30·27	74	61	62	71	55	60	50½	e.	e.	ne.	se.	·02	……	……	……
4.	29·977	29·926	29·44	30·16	30·12	30·33	30·32	78	60	63	72	53	50½	50½	e.	se.	ne.	s.	·03	……	……	……
☽	29·926	29·801	29·32	30·04	29·91	30·27	30·18	73	54	62	71	53	60½	50½	e.	e.	ne.	ese.	……	·45	……	·05
5.	29·934	29·837	29·22	29·86	29·80	30·04	29·81	76	54	63	65	54	50½	50½	sw.	calm	se.	se.	·01	·09	0·78	……
6.	29·913	29·894	29·33	29·76	29·70	29·68	29·95	77	55	61·5	69	57	50	46	sw.	calm	ne.	nne.	·28	……	……	·10
7.	29·873	29·840	29·34	29·78	29·77	29·92	29·77	79	55	58·5	60	57	49	51	se.	calm	sw.	sw.	·24	……	……	……
8.	29·747	29·717	29·20	29·70	29·65	29·68	29·56	66	54	57·5	61	53	53	50	w.	ne.	ssw.	w.	……	……	……	……
9.	29·954	29·820	29·20	29·69	29·84	29·92	29·78	69	40	56	61½	48	52	52	sw.	calm	w.	wsw.	……	……	……	·12
10.	30·014	29·995	29·48	29·88	29·83	29·64	29·65	73	44	58	61	49	57	51	n.	calm	s.	wsw.	……	·35	2·67	·16
11.	30·089	30·011	29·44	29·88	29·98	29·73	29·97	70	51	56	61	42	51	46½	w.	calm	e.	……	……	……	……	……
12. ●	30·159	30·138	29·55	30·01	29·96	29·70	30·14	73	51	57	58½	48	51	40	sw.	calm	e.	e.	·03	·20	……	·09
13.	30·078	30·030	29·52	29·83	29·78	30·07	29·71	74	56	60	63½	48½	49	48	sw.	s.	e.	e.	·05	·27	……	·19
14.	29·857	29·825	29·19	29·56	29·52	30·14	29·70	72	61	62	62	55	53	53½	se.	w.	e.	nw.	……	·09	……	……
15.	29·848	29·838	29·19	29·60	29·63	29·92	30·02	73	61	62	56½	54½	54	52½	w.	calm	e.	ne.	……	·11	……	……
16.	29·729	29·708	29·10	29·65	29·76	29·61	30·19	72	53	56	59½	54½	54	45	sw.	ne.	enc.	calm	·47	·54	……	……
17.	29·989	29·729	29·36	29·96	30·02	29·85	30·02	62	37	57	56	45	50	47	ne.	e.	ne.-n.	……	·11	……	……	……
18.	30·068	29·933	29·47	30·03	30·01	30·13	30·19	65	42	58·5	57	44	51	45	ne.	ne.	ne.	……	……	……	……	……
☽ 19.	30·028	29·997	29·47	30·10	30·12	30·20	30·25	66	40	56	56	43	51	42	e.	e.	ne.	……	·01	……	0·12	·08
20.	30·091	30·048	29·64	30·17	30·17	30·32	30·32	64	47	55	52½	39½	49	44	ne.	calm	ne.	se.	……	……	……	·28
21.	30·036	29·921	29·60	30·12	30·00	30·30	30·27	61	49	54	59	34	52	42	e.	calm	nw.	……	……	·40	……	·70
22.	29·741	29·692	29·30	29·90	29·90	30·22	30·13	62	49	55	59	36½	51	47½	ne.	calm	wsw.	sw.	……	·01	……	·12
23.	30·081	29·893	29·42	29·99	30·05	30·08	30·08	65	39	49	61½	46½	53½	51	ne.	ne.	sw.	wsw.	……	……	……	·10
24.	30·234	30·174	29·65	30·10	30·10	30·02	30·07	65	35	52	59	34	56	54	ne.	calm	sw.	sw.	……	……	……	……
25.	30·270	30·198	29·75	30·08	30·03	29·89	29·85	72	36	53·5	59	51½	52	55	sw.	calm	sw.	sw.	……	……	……	……
○ 26.	30·179	30·096	29·65	30·00	29·93	29·87	29·71	70	39	52·5	61½	52	49½	55	s.	calm	nw.	nw.	·05	……	……	……
27.	30·047	29·934	29·55	29·84	29·87	29·79	29·98	75	39	53	59	54	47	47	w.	calm	s.	w.	·01	·19	……	……
28.	30·219	29·961	29·48	30·09	30·17	30·03	30·20	59	30	46	58	37	47½	41½	ne.	n.	……	w.	……	……	0·12	……
29.	30·328	30·242	29·85	30·17	29·90	30·01	29·63	67	31	…	58	36½	47½	53	se.	calm	s.	w.	……	……	……	·10
Mean.	30·007	29·954	29·46	29·918	29·936	30·006	30·003	70·50	47·33	57·5	61·7	46·4	52·76	49·29					1·31	2·50	3·69	1·99

LXIX. *Observations on* Mr. Graves's *Theory of Imaginary Logarithms.* By J. R. Young, *Professor of Mathematics in Belfast College* *.

THE extended and accurate theory of imaginary logarithms, first propounded by Mr. Graves in the Philosophical Transactions for 1829 †, is pretty well known to the readers of this Journal, its merits having been very fully discussed by Mr. Graves himself in the volumes of the Philosophical Magazine for 1836 (S. 3. vols. viii. and ix.), in reply to certain objections that had been advanced against it. In these latter explanations of Mr. Graves, qualifications and concessions appear to have been made which I think were unnecessary, and which seem to me to deduct very considerably from the importance of his discovery. I cannot but consider that these modifications of the author have tended somewhat to retard the general reception of his improved theory; they certainly operated unfavourably on my own mind, and for a long time led me to entertain the conclusion, that what Mr. Graves had done was not a generalization of the previously existing theory, as it was left by Euler, but rather, to use his own words, "the erection of a wing" to the old building, which he seemed to regard as in itself complete, and as much as the original foundation could sustain.

In this brief communication it is my object to show that Mr. Graves's extended theory is necessary to complete the fabric which Euler had left unfinished; that it occupies no new ground, nor requires any amplification of the principles universally admitted before the publication of Mr. Graves's researches.

Let the arithmetical value of the Neperian logarithm of the real number a be represented by $l\,a$, and the general loga

* Communicated by the Author.   † See Phil. Mag. S. 2. vol. v. p. 378.

rithm, whatever it be, by $\log a$; then we know that all the values of a^x will be expressed by

$$a^x = 1^x \left\{ 1 + (l\,a)\,x + \frac{(l\,a)^2}{1\,.\,2}x^2 + \frac{(l\,a)^3}{1\,.\,2\,.\,3}x^3 + \&c. \right\};$$

and also that all the values of 1^x will be expressed by

$$1^x = 1 + 2k\pi\sqrt{-1} + \frac{(2k\pi\sqrt{-1})^2}{1\,.\,2}x^2 + \frac{(2k\pi\sqrt{-1})^3}{1\,.\,2\,.\,3}x^3 + \&c.\ [1.]$$

so that, combining this with the expression within the brackets, we have for a^x the following development, viz.

$$\left. \begin{aligned} a^x = 1 + (l\,a + 2\,k\,\pi\sqrt{-1})\,x + \frac{(l\,a + 2\,k\,\pi\sqrt{-1})^2}{1\,.\,2} \\ + \frac{(l\,a + 2\,k\,\pi\sqrt{-1})^3}{1\,.\,2\,.\,3} + \&c., \end{aligned} \right\} \quad . \quad [2.]$$

a development which is perfectly general.

From [1.] we have the following development of unity, viz.

$$1 = 1 + 2\,k\,\pi\sqrt{-1} + \frac{(2k\pi\sqrt{-1})^2}{1\,.\,2} + \frac{(2\,k\,\pi\sqrt{-1})^3}{1\,.\,2\,.\,3} + \&c.;$$

and this is the development we shall obviously obtain for a^x, provided we put $\dfrac{2\,k\,\pi\sqrt{-1}}{l\,a + 2\,k'\,\pi\sqrt{-1}}$ for x in [2.], and consider k in [2.], which is an arbitrary integer, to take only the values implied in k'. Consequently, under these restrictions, we shall have

$$a^{\frac{2\,k\,\pi\sqrt{-1}}{l\,a + 2\,k'\,\pi\sqrt{-1}}} = 1,$$

whatever be the value of a; and therefore when $a = e$, the Neperian base

$$e^{\frac{2\,k\,\pi\sqrt{-1}}{1 + 2\,k'\,\pi\sqrt{-1}}} = 1\ \therefore\ \frac{2\,k\,\pi\sqrt{-1}}{1 + 2\,k'\,\sqrt{-1}} = \log 1,$$

agreeably to the theory of Mr. Graves, whose steps I have imitated.

Now, in order to obtain the results of Euler, we may proceed from [2.] as follows; and no person, admitting the consistency of Euler's results with the common principles, can have any objection to this mode of arriving at them:—

For brevity we may write [2.] thus:

$$a^x = 1 + \mathrm{A}\,x + \frac{\mathrm{A}^2}{1\,.\,2}x^2 + \frac{\mathrm{A}^3}{1\,.\,2\,.\,3}x^3 + \&c.,$$

which, by putting $\dfrac{1}{\mathrm{A}}$ for x, becomes

$$a^{\frac{1}{A}} = 1 + 1 + \frac{1}{1 \cdot 2} + \frac{1}{1 \cdot 2 \cdot 3} + \&c. = e, \quad . \quad . \quad . \quad [3.]$$

$$\therefore a = e^{A} \therefore \log a = A = l\,a + 2\,k\,\pi\,\sqrt{-1};$$

and if $a = 1$, the first of these equations gives $e^{2k\pi\sqrt{-1}} = 1$.

It has been objected to Mr. Graves's exponential expression for 1, that that expression has innumerable other values besides the value 1; and that therefore any one of these has as much claim to the logarithm attributed to 1 as this 1 itself.

It is true that such innumerable values exist; since, in making the proposed substitution for x in [2.], the development represents 1, only when k is chosen equal to k'. But the very same is true as respects the more limited exponential form of Euler; the development [2.] becomes that of e, only when k is chosen equal to the k involved in the exponent of the first member of [3.]. In both cases, values, different from those retained, will be expressed by the exponential, if the k in [2.] be chosen of different value from the k in the denominator of x. Whatever objection therefore be brought against Mr. Graves's exponential, the very same may with equal propriety be brought against that of Euler; and if from any considerations it be overruled in the theory of the latter, the same considerations must be equally cogent in that of the former; and as Euler's form is only a particular case of that of Graves, the latter is the form to be used when the utmost generality is to be expressed.

In reality, however, the objection adverted to is untenable when applied to either theory; for although the exponential truly represents an infinite variety of different values, yet of only *one* of these values is the exponent itself the general logarithm. This is a fact which I believe has not heretofore been insisted upon; but that it ought to be insisted upon will, I think, appear from the following considerations.

Taking either of the two preceding exponential expressions for 1, let the other values involved in that expression be p, q, r, s, &c. Now of no one of these, p for instance, can the proposed exponent be called the general logarithm, because particular values of the arbitrary constant in that exponent, if Euler's be employed, or of pairs of values for the two constants, if Graves's be employed, can be chosen for which the exponential will fail to produce p. It is unquestionable that p has a general logarithm; but it is equally unquestionable, from this fact, that the proposed cannot be it; the only one of the series of values furnished by the exponential that can claim the exponent for its general logarithm is the 1, because this is the only value which the exponential *always* gives, whatever changes we introduce into the arbitrary constants; the simple circumstance that $k = 0$ renders the exponential incompetent to

express p, would be sufficient to exclude p from a claim upon the exponent for its general logarithm.

Mr. Graves says (Phil. Mag. S. 3. vol. viii. p. 281) " I call $\frac{1}{2}$ an e-log of $- \sqrt{e}$ as well as of $+ \sqrt{e}$." But this departure from the usual doctrine is not essential to the stability of Mr. Graves's system; and I submit that if, to subserve ulterior purposes, $+ \sqrt{e}$ be fixed upon, $- \sqrt{e}$ is at once deprived of its claim, the ambiguity remaining only till the selection is made; *both* cannot claim this designation, but only one or the other, just as in the case of $+$ and $-$ in the theory of coordinates.

These few observations may perhaps contribute to revive attention to Mr. Graves's interesting and masterly researches into the general theory of exponentials; and they may possibly help to show that the system of imaginary logarithms which he has established, is established upon precisely the same basis as that which was required to support the older imperfect doctrine, without the aid of any new principle or postulate.

Belfast, November 12, 1844.

LXX. *On the Probable Mode of Constructing the Pyramids ; introduced by Letters relating to the History of the subject, addressed to* Lieut.-Col. Sabine, Lieut.-Col. Dansey, *and the Author. By* HENRY PERIGAL, *Esq.*[*]

To Lieut.-Colonel Sabine, F.R.S., &c., General Secretary of the British Association for the Advancement of Science, York.

Sir,

IN offering for communication to the British Association for the Advancement of Science the accompanying description of a (supposed Egyptian) mode of raising very large stones, I take leave to annex copies of two letters, which explain that my discovery of this method of raising heavy weights had been anticipated by Lieut.-Col. Dansey, of the Royal Artillery; and to add that, in an interview I sought with him, I found that our plans were almost identical, and learned the gratifying fact that in 1834, when the Egyptian antiquities at the British Museum were transferred to the new gallery, under the personal direction of Col. Dansey, this method was partially adopted in the removal of "Young Memnon." Consequently no doubt need be entertained of its being *practicable*. The question for discussion, therefore, is whether such a plan was *actually practised* by the Egyptians in constructing the Pyramids, &c.   I am, Sir, yours truly,

Smith Street, Chelsea, Sept. 24, 1844.   HENRY PERIGAL.

To Lieut.-Col. Dansey, Woolwich.

Sir,—I called upon Dr. Faraday yesterday, to beg him to take charge of a paper I wished to present to the British Association for the Advancement of Science, on a mode of raising very ponderous stones, practised, as I imagine, by the Egyptians in the erection of their pyramids and temples; by the Druids in constructing Stonehenge; and in the location of rocking-stones, &c.; which I had re-invented in the month of May last. On seeing the explanatory diagrams I have had engraved, he told me he had some recollection that a plan on a similar principle (viz. by *tilting* the stone) was proposed by you, many years ago, for lifting heavy guns, &c.; and he recommended me to write to you about it.

Of course, under these circumstances, I feel constrained to pause, lest I should risk the imputation of plagiarism; I trust therefore you will kindly furnish me with the requisite information as to whether your contrivance had reference to the construction of the Pyramids, Stonehenge, and such like structures; and acquaint me in what Publications I can find an account of it. Its application to military purposes had not occurred to me, my thoughts not having any particular tendency that way; for a like reason it is not impossible, although perhaps not very probable, that you may not have contemplated its applicability to the Pyramids and Stonehenge. The elucidation of a difficulty that has puzzled the world for more than two thousand years is worthy of some credit; and I should be glad to acquire that to which I am fairly entitled by my discovery, if I can do so without infringing upon or appearing to usurp any share of what is due to your acknowledged priority. Had I known I was anticipated, I should have saved a good deal of time and trouble, and been spared some little expense and mortification.

The British Association meet at York next week, on the 26th instant, I believe; consequently, the sooner you favour me with a reply the more you will oblige,

Yours truly,

5 Smith Street, Chelsea, Sept. 19, 1844. Henry Perigal.

To Henry Perigal, Esq.

Sir,—I have had the pleasure to receive your favour of yesterday. In reply I beg to state, that my method of raising weights has long been in practice in the artillery drills, and I do assure you that my imagination has always dwelt upon the applicability of it upon the most extended scale, from lifting a gun on to its carriage to raising a line-of-battle ship in a dock; not excluding Luxor Monoliths, Stonehenge Architraves, &c. &c. &c.; and it was with reference to such general applica-

bility, beyond the sphere of military purposes, that I mentioned it to Dr. Faraday. I wrote to him yesterday, in consequence of his having written to me upon the present occasion, and I doubt not he will have great pleasure in showing you my letter and diagram.

I trust that the time and trouble you have bestowed upon the subject may not be without their fruits; the illustrations by your diagrams will not lose their value, nor will the fact of its having slumbered a quarter of a century in the exclusive exercises of the artilleryman render less interesting the public demonstration of a principle which had been buried a hundred times as long in the Pyramids of the Ptolemies and Pharaohs.

Royal Laboratory, Woolwich,   I am, Sir, yours very truly,
     September 21, 1844.   C. C. DANSEY.

Part I. *Description of a Process supposed to have been adopted by the Egyptians to raise the Stones from step to step in the Construction of the Pyramids.*

For two or three thousand years, or more, it has been a matter of wonder, and a favourite subject of surmise and inquiry, how the ancient Egyptians contrived to elevate to their places the enormous masses of stone of which the Pyramids are constructed; such being the magnitude of these structures and the supposed difficulties surmounted in their erection, that the great Pyramid was designated one of the seven " wonders of the world." What kind of engines were employed (if any were used), or what expedients were adopted to raise the ponderous stones, has continued a mystery to the present time; although various methods have been suggested, more or less practicable, not one of these conjectures has been considered a satisfactory solution of the problem which has baffled the learned and ingenious for so many ages.

Chronologers assert that the great Pyramid of Gizeh is from 4000 to 5000 years old. Herodotus, who visited Egypt about 2200 years ago, gives the following account of its erection, as described by the priests who then had charge of the Pyramids :—

" They told me likewise that Cheops, who succeeded Rhampsinitus, oppressed the Egyptians with hard labour; appointing some to receive the stones that were dug out of the quarries in the Arabian mountains and to convey them down to the Nile; and when they had been transported in vessels to the other side of that river, he appointed others to receive them, and to drag them to the mountain called the Libyan. Ten years were spent in constructing the causeway along which they drew the stones. Twenty years were spent on the Pyramid, which is quadrilateral, every face containing eight

plethra* in length, and the same measure in height. · All the stones† are 30 feet long, well-polished, and joined together with the greatest exactness. This Pyramid was built in successive layers in the form of steps, like an altar. When they had commenced in that manner they superposed other stones by means of machines consisting of short pieces of wood, raising them first from the ground to the first range; when the stone arrived there it was put on another machine, which rested on the first step, from which it was raised to the second, and so on, for the engines thus employed were equal in number to the several ranges of stones; or perhaps there was but one engine, which, being easily managed, might be removed as often as they deposited a stone; for I must mention both ways, as related to me. The summit was first completed‡, and the rest in succession, so that the last of all finished were the lowermost parts nearest the ground." (Herodotus, b. ii. ch. 124 and 125.)

Although the stones of the great Pyramid are not all 30 feet long, as Herodotus asserts, yet some of them are more than 40 feet long; and in the middle Pyramid of Abouseir the roof of one of the apartments is formed by three tiers of blocks, each block $48\frac{1}{2}$ feet in length. In the ruins at Baalbec are "three stones elevated nearly 20 feet from the ground, each measuring 70 feet in length by 15 in width, while in the quarries, about a mile from the city, there still remains one enormous block, smoothed, planed, and ready for removal; it measures 70 feet in length, 14 feet in height, and is 17 feet in thickness at one end and $13\frac{2}{3}$ feet at the other, being the same shape, but larger than those in the wall." Mr. Wood computed its weight at 2,270,000 lbs., or 1135 tons!

There appears to be no evidence to prove that the architects of the Pyramids were acquainted with any contrivances or combinations equivalent to what would be called machines or engines, according to the modern acceptation of the words; on the contrary, it seems much more probable that their gigantic undertakings were accomplished by some very simple means; which simplicity (leading to the notion that the means were self-evident) was perhaps the very reason that no record was kept, or transmitted to posterity, of their mode of operation. With this conviction, on the assumption that the statement of Herodotus might be founded on fact, I endeavoured to discover in what manner such prodigious blocks *could* have been elevated, from step to step, *merely by the aid of short*

---

* 808 feet.

† Meaning, probably, the casing-stones, or polished stones with which it was faced.

‡ *Faced* with polished stones.

pieces of wood, when the idea occurred to me that they *might* have been so raised by some such system as the following process :—

Each block of stone, shaped and prepared for use before it left the quarry, was conveyed across the Nile (advantage being taken of the periodical inundations) on rafts, or other appropriate vessels, to the causeway described by Herodotus; along which it was dragged on rollers, or on sledges if the stone was smoothed or polished, by the labour of men (or of cattle), to a convenient locality adjoining the Pyramid, where it remained till wanted; thence it was conducted to the first step of the Pyramid on rollers. To get the rollers underneath wedges were used, if it lay on the hard rock; otherwise the earth was removed from beneath one half of the stone, the director or superintendent having placed himself upon the further end to prevent it from tilting over too soon.
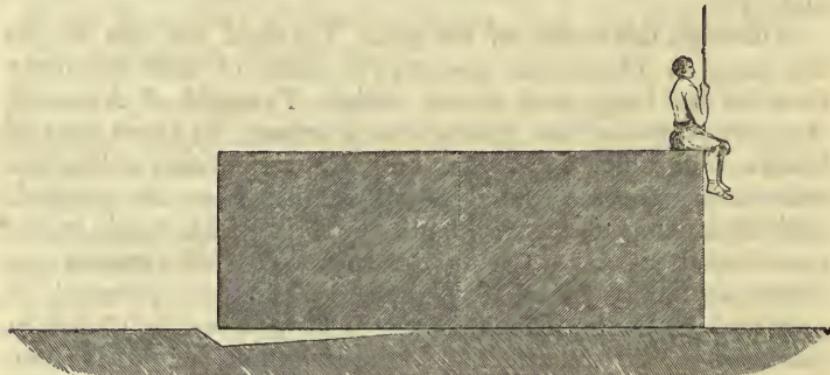
Next, the director having walked on the top to the other end, the stone (overbalanced by the leverage of his weight)
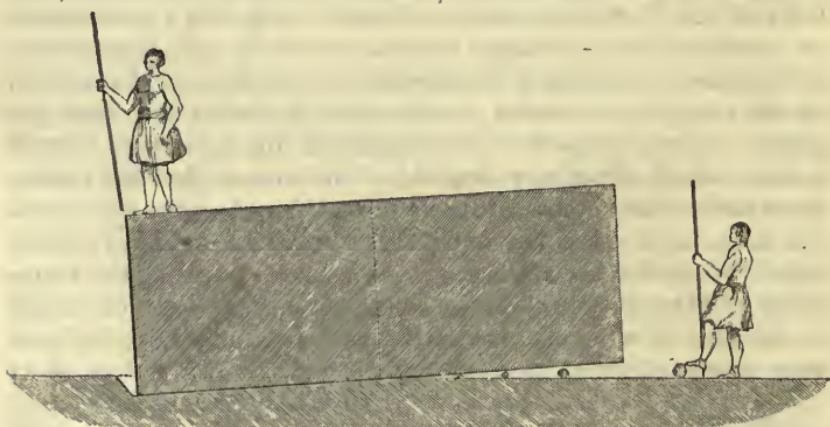
tilted into the hollow in the ground, when rollers were placed under the other half of it.

The director having walked back again the stone was tilted on to the rollers, and conveyed to its destination at the foot of the Pyramid; where, perhaps, it was transferred in a similar way to larger rollers.

Then commenced the lifting process. All but one roller being removed, that one being as nearly as possible under the centre of gravity, the stone was tilted as before, while flat boards or planks were placed beneath; and upon these boards another very much narrower to act as a fulcrum, all being about the same length, proportioned to the width of the stone.
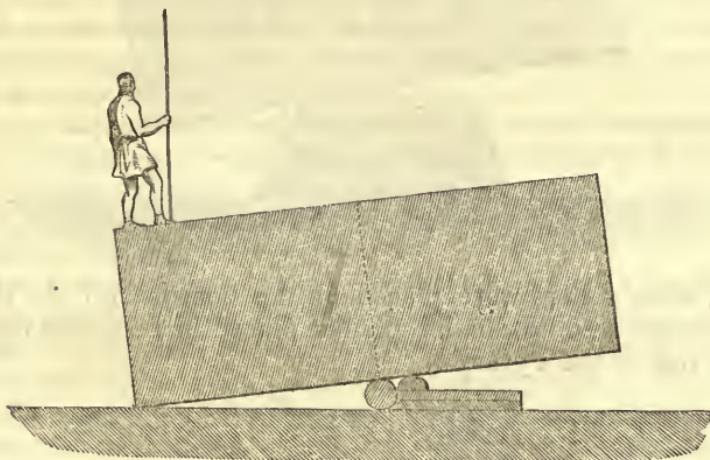
The director having walked to the other end the stone was tilted on to the boards, and similar planks were piled beneath by the side or parallel to the others, but a degree higher or more in number; and upon them also a narrow fulcrum-slip, upon which the stone was then tilted.
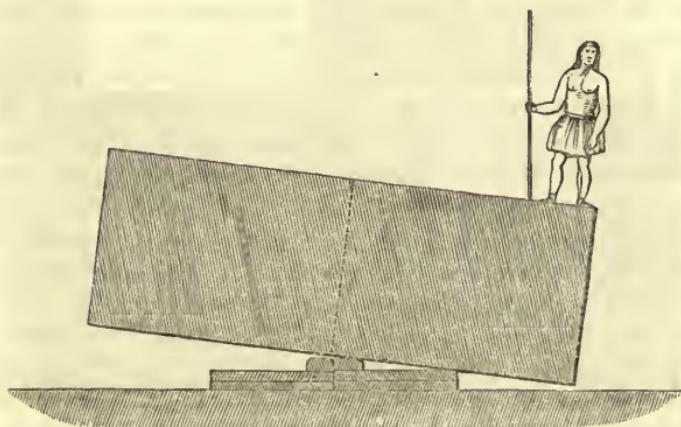
The director having repeatedly walked backwards and for-

wards, tilting each end of the stone alternately, and additional boards having been introduced every time, the stone gradually rose to the required height, rather exceeding that of the next
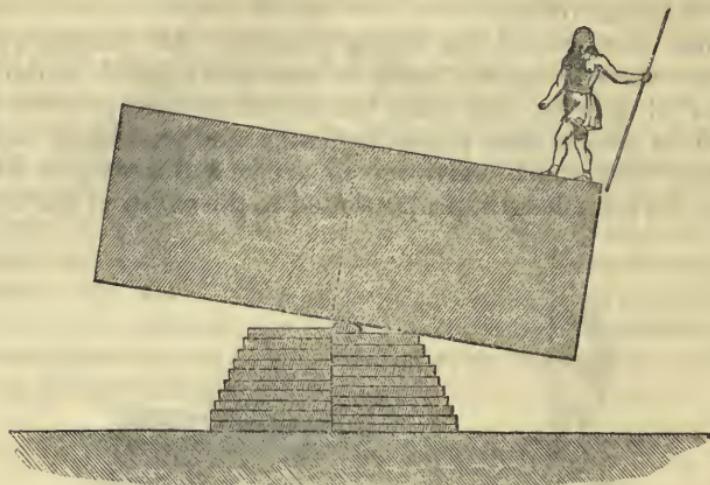
step, when rollers were placed on the boards and the stone was transferred to similar planks placed in readiness on the next step of the Pyramid.
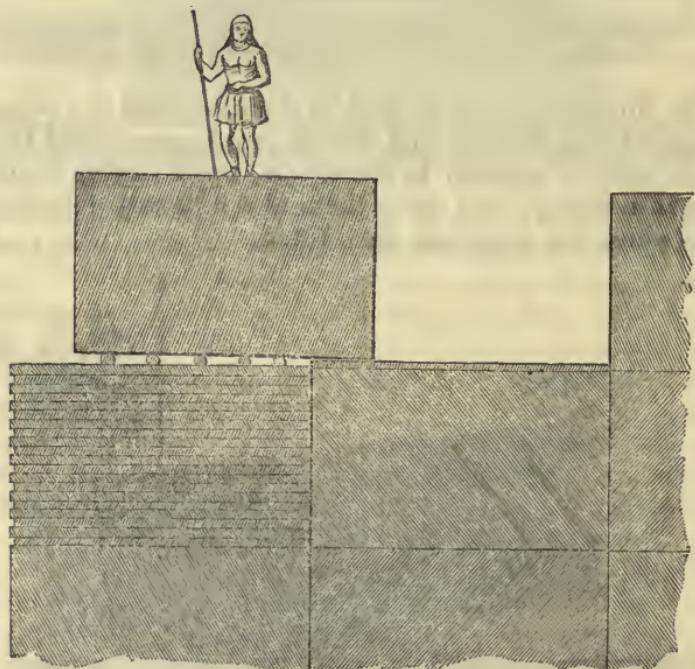
The same process was then renewed, and continued from step to step till it arrived at its destined locality.

[N.B. In the diagrams the thickness of the planks and the consequent slope are exaggerated to make the operation more distinct and evident. The last diagram is a cross section, the others are longitudinal sections.]

Should any of the stones have been short, and consequently have afforded insufficient leverage for one man's weight to tilt them, he might have carried a load ; or planks might have been made fast at the top so as to project beyond the ends of the stone for him to walk along; or two or more men might have been employed in traversing the stone; or various other expedients might, obviously, have been adopted to tilt the stone. The wood probably underwent some preparatory process by which it was condensed and its elasticity destroyed, perhaps by being subjected to very heavy pressure when sodden with boiling water.

Thus "the properties of the lever and of the centre of gravity were brought into co-operation, so that *the weight to be lifted was itself the principal element of the lifting power.*" Figuratively speaking, THE STONE WAS MADE TO RAISE ITSELF BY MEANS OF ITS OWN WEIGHT.

In this manner, with the aid of a few dozen planks, a couple of men (one traversing the stone while the other arranged the planks) might have conducted to the top of the great Pyramid the largest stone used in its construction ; thus corroborating the assertion of the Egyptian priests, as stated by Herodotus, that the "*stones were raised from step to step by the aid of short pieces of wood*; *which, being portable and easily managed, might be removed or transferred as often as they deposited a stone ; or different sets might have been employed for every range of steps.*" By this simple process, also, a few men might have raised Stonehenge in a single night, if the requisite stones were provided and placed in readiness near the spot, without any previous or subsequent indication of the means by which it was effected ; affording the Druids a favourable opportunity of practising upon the ignorance and credulity of the multitude by ascribing its erection to supernatural agency.

In conclusion, I may add, that in looking over a good many works on Egypt and its antiquities, I have not succeeded in meeting with any direct proof that this system was the very method, or one of the methods, actually practised by the Egyptians; but perhaps the following quotation may be interpreted into something like presumptive evidence of its probability, as the fracture of the obelisk *in the middle,* as there described, is an accident very likely to have occurred in the attempt to move the shaft by such a process, if the stone happened to be brittle, or its breadth and thickness too small for its length. "In one of the quarries at E'Sooan (Syene) is a granite obe-

lisk, which, having been *broken in the centre* after it was finished, was left in the exact spot where it had been separated from the rock. The depth of the quarry is so small, and the entrance to it so narrow, that it was impossible for them to turn the stone in order to remove it by that opening; it is therefore evident that they must have lifted it out of the hollow in which it had been cut, as was the case with all the other shafts previously hewn in the same quarry. Such instances as these suffice to prove the wonderful mechanical knowledge of the Egyptians; and we may question whether, with the ingenuity and science of the present day, our engineers are capable of raising weights with the same facility as that ancient people." (Manners and Customs of the Ancient Egyptians, by Sir J. G. Wilkinson, F.R.S., vol. iii. p. 332.) With the latter opinion I cannot coincide; I am confident that some of our engineers could manage to construct a pyramid as large as that of Cheops, with as large stones, in a tenth part of the time, with a hundredth part of the number of workmen, if they were not limited in its cost.

Smith Street, Chelsea, September 24, 1844.

LXXI. *On the Fish River of the Polar Sea.* By RICHARD KING, *M.D.*[*]

FROM the period when Hearne, under the guidance of the Indian chief Motannabee, undertook his memorable journey to the Polar Sea, we have been acquainted with the source of the Thlewydezza, or Fish River. By this stream it was proposed, in 1833, to reach the wreck of the Fury, in search of Captain Sir John Ross and his party. In a short paper read before the Royal Geographical Society, and published in the third volume of its journal, the river is described, upon the authority of the Indian Blackmeat, "as being well adapted for the winter residence and support of an exploring party, both on account of its woods and the game that resorts to them; that the access to it from Great Slave Lake is easy, the water communication being interrupted by only three short portages; that the debouche is into or opposite Regent's Inlet, which points it out as well adapted for the starting-point of a boat expedition in search of the crew of any vessel known to have had the intention of visiting the wreck of the Fury." In

confirmation, a sketch of the country between the Great Slave Lake and the Fish River, wherein is accurately laid down the three portages interrupting the water communication, is published in the official narrative of the Arctic Land Expedition in search of Captain Sir John Ross and his party. We are further informed in the same narrative, that the Indian guide Mauffly repeatedly, while on duty, pointed out well-known routes to the river, not only from Great Slave Lake, but from Lakes Aylmer and Clinton Colden. It does not appear that Captain Sir John Franklin had made up his mind upon the subject when the river and its trendings were under anxious consideration at the time of the contemplated journey in 1833, in search of the lost travellers, but by a written communication in 1836 he informed the Royal Geographical Society * that he did not believe the river fell into the Arctic Sea. " The river," he says, " does not, it is imagined, flow to the northward, but to the eastward, and probably falls into the sea in Knapp's Bay, or between the Chesterfield and Wager Inlets. I doubt," he continues, " the finding of fuel on the banks of the river or near the source of it, as I always understood that part of the country to be destitute of wood. I much doubt also the possibility of a party getting a sufficient quantity of fish in any river in that quarter to support it during a winter."

The existence of a river running from the vicinity of Great Slave Lake into Regent's Inlet is of such vast importance in a geographical, and as far as the Hudson's Bay Company is concerned, in a commercial point of view, that it is necessary to well weigh Captain Sir John Franklin's doubts as a test of their value. It is necessary, however, to remove an impression, pretty generally prevalent, that the exploring party in search of Captain Sir John Ross and his party discovered the river of which it went in search. This is not the case; for it was portrayed as falling into the sea of Regent's Inlet, having on the one side Melville Peninsula, and on the other a high promontory running far to the north (the land of North Somerset?), as being well adapted for the winter residence and support of an exploring party, the country in its vicinity abounding in animals and the lakes in fish, and that source separated from Great Slave Lake by three portages. But these are not the features of the river discovered by the exploring party; on the contrary, they are just the reverse; for in the first place not a particle of wood grows either at its source or throughout its whole length, and therefore it is not

* Sir John Franklin's report here referred to will be found in Dr. King's former communication, Phil. Mag. S. 3. vol. xx. p. 488.

adapted for the winter residence and support of an exploring party; 2ndly, it does not fall into an inlet, having Melville Peninsula on the one side and land running far to the north on the other, but into a gulf to the westward of that inlet, in which the far northern land lies on the right instead of the left hand; 3rdly, it has not three portages between it and the Great Slave Lake. It is evident, therefore, that it is in the name alone that there is any similarity between the river for which the exploring party sought, and that which they found; and it is equally evident that in the paper read before the Geographical Society it is the Fish River, and not the Great Fish River, which is there described. In confirmation, there appears in the official narrative of the expedition in search of Captain Sir John Ross, published in 1836, the engraved chart of a copper Indian, wherein the position, trending and separation by three portages of the Fish River from Great Slave Lake will be found exactly to correspond with the chart published in the Royal Geographical Society's Journal. We have the testimony, moreover, of Dr. Richardson, that the Fish River is well known to the fur traders, and that they travel to it from the Athabasca in four days; and we have the further testimony of the fur trader, Roderic Macleod, who accompanied the exploring party. Additionally, George Sinclair, one of the most able men attached to the expedition in search of Captain Sir John Ross and his party, and more recently to that of Messrs. Dease and Simpson, while on an excursion for provision, crossed the tributary to the Great Fish River, said by the Chipewyan chief, the Comrade de Mandeville, to take its rise near the head waters of the Fish River. That tributary was seen by him trending in a direct course for the Great Fish River through a well-wooded country, and affording everything necessary for a winter establishment.

Surely after such testimony we can no longer doubt the existence of the Fish River; that it takes its rise in a well-wooded district, and that it flows parallel with the Great Fish River to the Polar Sea. The facts I have recorded, the extreme accuracy of the Indians regarding the Great Fish River, too well known to need repeating here, the northerly trending of the eastern boundary of the estuary into which the Great Fish River falls, the southerly trending of North Somerset, all combine to induce me to put implicit belief in the original paper laid before the Geographical Society, that the Fish River falls into an extensive gulf, whose west side is bounded by a promontory running far to the north, and its east side by Melville Peninsula; and that finally that gulf will be found to be identical with Regent's Inlet. That the Fish River

empties itself into that gulf is rendered more probable from the fact, that drift wood is found, according to Esquimaux in-formation furnished to Captain Sir Edward Parry, at Siatoke, one of the islands of that gulf; and whence could this wood come, but from a wooded country? and the Fish River is the only stream eastward of the Mackenzie that is known to take its rise within the limits of the woods.

Now Captain Sir John Franklin "imagines that the Fish River does not flow to the northward, but to the eastward, and probably falls into Knapp's Bay, or between the Chester-field and Wager Inlets." Surely the gallant officer will no longer entertain this belief. Again, he doubts the finding of fuel on the banks of the river or near the source of it, as he always understood that part of the country to be destitute of wood. Everybody except Captain Sir John Franklin, as far as I know, always understood the contrary; besides, the known confines of the woods, extending as they do, according to Dr. Richardson, to $63\frac{1}{4}°$, sets the matter entirely at rest, a fact altogether overlooked by Captain Sir John Franklin. Further, he doubts the possibility of an exploring party get-ting a sufficient quantity of fish in any river in that quarter to support it during a winter; but who proposes to search for fish in the head waters of rivers? It is in the lakes and not in the sources of rivers that fish are to be found in the winter season, as is well known to the Indians and the fur traders. After weighing the evidence on all sides, we can hardly, I think, come to any other conclusion than that the Fish River has been accurately described in the Royal Geographical So-ciety's Journal. It is evident then that it must form a promi-nent feature in all further attempts to survey the unexplored polar lands. It is the high road by which to connect the discoveries of Messrs. Dease and Simpson on the one side with those of Captain Sir Edward Parry on the other. The sea of Regent's Inlet can thus be traced upwards, its bound-aries on either side can be explored; with Melville Peninsula we shall thus be made thoroughly acquainted, and the insular or peninsular character of North Somerset will be determined.

Considerable importance has been attached to the land of North Somerset, from a belief that it is an island, which, if proved, would at once solve the grand problem of three cen-turies—the discovery of the north-west passage. This is evidently an error; for, if insular, its separation from the main land can be but of trifling extent, otherwise there would be a strong current setting through the Fury and Hecla Strait, whereas, according to Captain Sir Edward Parry, there is no current; while the absence of any current through that

strait is a powerful argument in favour of its being a peninsula. Whence then comes the mighty current setting down Davis's Straits, which drove that great "marine worthy," John Davis, some 500 miles in ten days? Certainly not from Lancaster Sound or the Fury and Hecla Strait. To the north then of the former sound we must search for the water communication connecting the two great oceans, and the most likely road is by Jones's Sound. This opening alone arrested the attention of Baffin, the discovery of the bay named after him, although he passed others, including Lancaster Sound, in the belief that they were bays. Since Baffin, Captain Sir John Ross is the only traveller who has visited Jones's Sound, and as he was wrong with respect to the character of Lancaster Sound, we cannot place much confidence in the rest of his survey, while we have the southerly movement of the waters of Baffin's Bay unaccounted for.

In conclusion I would observe, that although to fix the North American boundary of the Polar Sea ages have been required, owing to the apparently insurmountable difficulties in the way, the prosecution of the survey has led to adventures so fruitful in incident and so instructive in character that we are able to recur again and again to the subject. And shall we now stop, when one season, one short summer, is all that is required to complete the labours of three centuries?

LXXII. *Observations on the Circular Polarization of Light by transmission through Fluids.* *By* H. B. LEESON, M.D., A.M.*

IT is with considerable diffidence, and not without some regret, that I present the following observations on the circular polarization of light by transmission through fluids.

It is with diffidence, because the conclusions, which are the result of my own experiments, differ from those of others, deservedly considered able and acute observers. It is with regret, because the tendency of the facts, which I expect to establish before you, is to lessen in some degree the value of an otherwise beautiful application of optical science to the exigencies of chemical investigation.

It is equally important to detect that which is erroneous as it is to establish that which is true in science. Truth being my only aim, I trust that, whilst endeavouring to remove the

---

* Communicated by the Chemical Society; having been read December 4, 1843. As inserted in the Memoirs and Proceedings of the Society, vol. ii., this paper is accompanied by a series of plates, the omission of which has occasioned some slight verbal alterations, which the author has had the kindness to revise, having also inserted some additions and corrections.

veil of mystery and obscurity which has hitherto perplexed and thwarted many who, in common with myself, have assayed to follow out the experiments detailed by Biot and others, I may not be considered anxious to throw discredit on their labours; but candidly detailing the result of my own experiments, I can only say to them, as well as to others, " Si quid novisti rectius istis, candidus imperti; si non, his utere mecum."

The point in which the results of my own experiments disagree with those of other observers, is in regard to the alleged possession of *opposite* rotations by *certain* fluids, which have the property of deviating the plane of polarization of a beam of polarized light transmitted through them, some of those fluids being said to produce a *right*-handed rotation, others a *left*-handed rotation. On examining a great variety of samples of the *particular* fluids said to possess opposite rotations, I found that such fluids deviated the plane of polarization in only one direction, namely, towards the *right*-hand. The only fluids in which I have hitherto been able to discover a decided *left*-handed rotation is in the essential oil of lavender, and in that of cubebs*. The interesting fact, if true, that solutions of sugar, when obtained from different sources and in different conditions, possess in some cases a right-handed and in others a left-handed rotation, has not been corroborated by my own observations. When such solutions did exhibit any *decided* rotating influence, it was uniformly right-handed. It is, however, curious that solutions of cane-sugar differ from those of grape, potato and diabetic sugar in this respect; that whilst the former possessed a powerful rotating influence, the latter exhibited little or no rotating energy†, although their solutions were in some cases sufficiently concentrated to furnish crystals after a few days' repose.

It might perhaps be supposed that such result proceeded only from my own want of information on the subject, or a deficiency of my powers of observation; indeed, I long so considered it myself, so much so, that, not satisfied with observations made with apparatus procured in this country, I com-

---

* The amount of left-handed rotation in oil of cubebs is much greater than that of oil of lavender. This oil (which was colourless), and some others contained in the table accompanying this communication, have been examined since my paper was read, the samples having been kindly furnished by Mr. Warington of Apothecaries' Hall. Samples of oil of cubebs obtained elsewhere were coloured, and possessed much less rotating energy, being probably adulterated with oil of peppermint.

† Although possessed of no *rotating* energy they have a *depolarizing* influence, as subsequently described.

missioned a friend to procure for me from Mons. Soleil of Paris an apparatus made according to Biot's own directions. Experiments made with that instrument confirmed the conclusions I before arrived at, and I may add, that an examination of the apparatus itself has in some degree suggested the possible sources of error.

I do not of course positively assert that the observations of others have been erroneous, although strongly inclined to suspect they may have been misled. Nothing but an examination of the *same* sample could indeed establish such conclusion. I have never, however, been so fortunate as to obtain a solution of sugar possessing a decided *left*-handed rotation; and although it is certainly possible that the oil of lemons or oil of turpentine which I have examined may differ from that experimented on by others, still I have tried so many samples, warranted genuine and procured from so many different sources, that I can scarcely imagine it possible to procure samples, obtained from the same species of tree, possessed of opposite rotating powers ; I may also remark, that although I have observed variations as respected the amount, I have never noticed any variation as respected the *direction* of rotation in any of the samples of the same description of fluid*.

Misunderstanding may perhaps result from the converse use of the terms *right*- and *left*-handed rotation by different writers, but this cannot affect the essential fact, if true, that oil of lemons, for instance, has an opposite rotation to that possessed by oil of turpentine, or the still more interesting fact alleged, unfortunately, I fear, incorrectly, that a solution of sugar as existent in the juice of grapes rotates the plane of polarization in an opposite direction to that in which it is rotated by a solution of sugar obtained from the cane.

The direction to which, in common with other writers, the term right-handed is subsequently applied cannot be mistaken by those who attend to the instructions hereafter given, and I would recommend every one, previously to experimenting on fluid substances, to study the opposite appearances presented on analysing polarized light when transmitted through specimens of right- and left-handed quartz, as usually sold for illustrating circular polarization.

With a view to the more explicit understanding of the mode in which the experiments referred to have been conducted, and to assist those not conversant with the subject in repeating them for themselves, I shall very briefly explain what is

* Since this paper was printed in the Memoirs of the Chemical Society I have obtained from Paris a specimen of "Laurel turpentine," as it was termed, possessing left-handed rotation.

meant by the deviation or rotation of the rays of polarized light when transmitted through fluids said to possess circular polarization.

Assuming the general principles of the undulatory theory of light, and making use of terms which, if not absolutely correct, are at least convenient, I shall first advert to what is meant by *rectilinear*, or, as I would suggest it should be termed, *rectangular* polarization.

When a beam of ordinary light impinges on a polarizing medium, it may be considered as dividing itself into two portions, the waves of which respectively undulate in directions varying from each other 90°, *i.e.* a right angle, in other words, they are said to possess opposite planes of undulation, and to be rectangularly polarized*. This effect may be produced by five different modes, viz. refraction, reflexion, absorption, dispersion, and double refraction.

A number of pieces of thin glass † superimposed on each other, and inclined at a suitable angle to a ray of light incident thereon, polarizes one portion of the light transmitted through it by refraction, whilst the other portion reflected from the surface is also polarized, but in an opposite direction. That the two portions into which the light is thus divided are oppositely polarized, or may be supposed to undulate in opposite directions, is established by the fact, that if transmitted towards a similar bundle of glass, the two rays would comport themselves differently; that is, they would not both be reflected or both transmitted by such second bundle if held in the *same* direction or plane towards each respectively. Calling the one portion of polarized light into which the original beam of ordinary light R (figs. 1 and 2, p. 420) is divided O, and the other portion X, it will be found that the position of the second bundle necessary to reflect X must be at right angles to that in which it would reflect O, and similarly as to refraction or transmission. This second bundle, serving to distinguish the two rays, is termed the analyser. Figs. 1 and 2 serve to illustrate polarization by refraction and reflexion. In fig. 1 the bundles of glass are supposed to be all in the same plane, or situated parallel to each other, whilst in fig. 2 the bundles B and C are supposed to be situated at right angles to the bundle A. The beam of light R impinging on

* Consideration of the direction in which the waves of light may be supposed to undulate will materially assist the memory in studying the facility of reflexion or transmission of the respective rays by surfaces opposed to them in different positions.

† The very thin glass sold for covering microscopic objects is extremely convenient for this purpose.

the bundle A is divided into two portions, O and X; and it will be observed that O, proceeding in its original course, is transmitted or passes through the second bundle B, fig. 1, whilst X is reflected from, and does not pass through, the bundle C. The converse takes place in fig. 2. Hence it will be observed that in fig. 1 the portion O is capable of

Fig. 1.
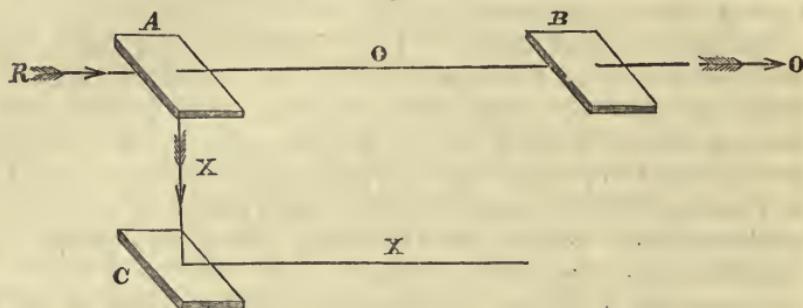
Fig. 2.
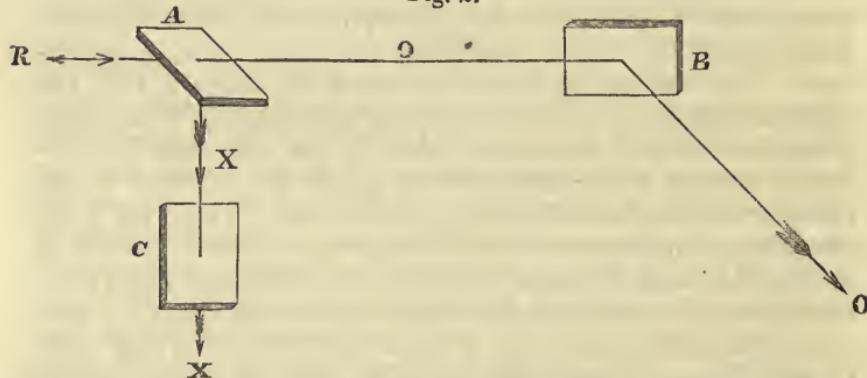
transmission but not of reflexion, whilst the portion X is capable of reflexion only. In fig. 2, where the bundles B and C are supposed to be placed at right angles to the bundle A, the portion O is capable of reflexion only, and X of transmission. In these figures A forms what is termed the polarizer, whilst B and C are what are termed analysers, serving to test the condition of O and X.

A tourmaline effects a similar division of a beam of light impinging on it, into two portions oppositely polarized, transmitting one, say O, in one position, whilst X is absorbed: revolving the tourmaline 90°, X will be transmitted and O absorbed. For experiments on circular polarization, as well as for microscopic purposes, the green tourmalines may be recommended, as they can be more easily obtained free from flaws. When of a proper thickness and ground truly parallel,

they form excellent analysers to apply to the eye-pieces of microscopes; for experiments on circular polarization they form very useful polarizers. The blue, yellow and hair-brown tourmalines are perhaps superior when free from flaws, but cannot be so readily obtained perfect.

Agates polarize by dispersion, but are not suitable for experiments on other substances.

Iceland spar and quartz polarize light by double refraction. The beam of light transmitted through them is divided into two portions, O and X, polarized in opposite planes, but proceeding in paths so closely posited, that they may both be the subject of observation at the same time. This is not the case however in what are termed Nicol's prisms, which are purposely so constructed as to allow only one portion to be transmitted when held in one direction, whilst the other is transmitted when the prism is revolved a right angle. The rhombs of Iceland spar are usually rendered achromatic by cementing a piece of glass properly adjusted to them. In order to render quartz available a particular construction is necessary, but when well-made, double refracting analysers of quartz are equally useful for these experiments.

It does not matter by what means our polarized beam is first obtained, or in what manner the same is analysed, or whether one mode be used to polarize the light and another to examine or analyse the same. In all cases the result will be dependent on the relative positions of the polarizer and analyser, and if these are placed in such a direction to each other as that X is not transmitted to the observer's eye, and therefore dark, then O will be transmitted; but if either the analyser or polarizer is changed to a position exactly at right angles to its former direction, then O will be obscured, whilst X becomes light. When one portion has obtained its greatest obscuration, or is invisible, the other will have obtained its maximum of illumination, whilst they pass gradually from one condition to the other at the intermediate positions*.

* A most convenient mode of obtaining polarized light which occurred to me about two years ago, and which I have since shown to several friends, consists in placing an ordinary looking-glass on the outside of a common window, the bottom of the mirror being placed close to the lower portion of one of the panes of glass, whilst the top is inclined from the window towards the sky, so as to reflect the pane of glass into a horizontal position. Standing then within the apartment, and looking through a tourmaline or other analysing medium at the light of the sky reflected from the mirror through the pane of glass, it will be found intensely polarized, and on introducing any substance, such, for instance, as a fluid contained in a glass tube or a crystal, &c., between the analyser and the polarizer, it may be readily examined. The great advantage of this mode of obtaining polarized light consists not only in the perfection of the polarization, but also

On using as an analyser a rhomb of Iceland spar, or a prism of quartz constructed as before alluded to, to examine a ray of polarized light transmitted through a small circular aperture, the oppositely polarized beams of light, O and X, may be observed together, and they will become alternately light and dark as the analyser is revolved, according to the law already laid down. The ordinary ray being represented by O, and what is termed the extraordinary ray by X, the rhomb or prism should be so arranged or posited in its setting as that O be central, and therefore apparently stationary, whilst X revolves around O*, as the analyser is itself rotated. Each portion, O and X, returns to its original condition after a revolution of half a circle, or 180°, and it passes from the state of greatest obscuration to that of maximum illumination on a rotation of 90°, or quarter of a revolution.

In all these cases, then, of what is termed *rectangular* polarization, the maximum of illumination takes place when the analyser is placed with its plane of polarization corresponding to that of the ray to be transmitted, whilst such ray attains its greatest obscuration when the analyser is placed at right angles to such plane. Now this condition is altered when a medium of a certain thickness †, possessing what is termed *circular* polarization, is introduced between the polarizer and analyser; for if the polarizer and analyser be placed so that one ray, X, is at its greatest obscuration or invisible, and the other, O, at its maximum of illumination, then on introducing

in the extent of the field of view, which is a great advantage in examining large specimens, such as unannealed glass, although not so important in circular polarization.

I may here observe, that the sky at all times furnishes polarized light, which is more or less intense according to the relative position of the observer and the sun. This circumstance probably influences the chemical action of the rays of light, and points out the necessity of varying the position of the camera, or the period of exposure in taking Daguerreotypes and other optical investigations, according to the time of day, the state of the sky, and the sun's declination. Any person may readily ascertain where the greatest intensity of polarization exists by using a tourmaline, or other analysing medium, and looking through it at a piece of selenite, or a piece of Iceland spar or other crystal, cut to exhibit the cross and rings; of course the colours will be most vivid, and the black cross most intense, where the greatest polarizing force exists. This polarization is most intense where the sky is blue and unclouded. The planes of polarization appear to vary at a total distance of 90°, or 45° measured each way from the sun's position.

* This is very important to prevent confusion arising from a revolution of the two images around each other, whereby the one may be mistaken for the other.

† A certain thickness is here stated, because, as will be hereafter shown, it is possible in every case to employ a thickness which would produce no apparent alteration.

a circularly polarizing medium between the analyser and po-
larizer, the plane of polarization will be removed to the right-
hand or to the left. In consequence of such change in the
plane of polarization, X will become lighter and O darker.
When the rotation of the analyser *proceeds in the direction of
the numbers* in the ordinary mode of graduation, the rotation
is termed *right*-handed, when in a contrary direction *left*-
handed.

Supposing the rotation produced be 30° to the right, then
the analyser must be also shifted 30° to the right-hand, in
order to obscure X; proceeding onwards, it will be observed
that X becomes bright at 120°, being a further revolution of
90°; again becomes obscured at 210°, or a revolution of 180°,
and again bright at 240°. On registering, then, the degrees at
which O and X are respectively polarized, that is, most ob-
scured, we have the following table:—

X	O	X	O
30°	120°	210°	300°

the degrees being observed on a right-handed graduated circle,
and corresponding to

X	O	X	O
330°	240°	150°	60°

on a circle graduated in a left-handed direction.

Had we moved the analyser in a left-handed direction, we
might therefore have registered our results as follows:—

O	X	O	X
60°	150°	240°	330°

by which it will be seen that it would be impossible, from the
appearance presented at any given position, or the direction
in which the analyser was revolved, alone considered, to say
whether the deviation was 30° to the right or 150° to the left;
and this would of course be equally true of any other amount
of deviation.

To add to the difficulty, the deviation is in direct propor-
tion to the thickness and nature of the circularly polarizing
medium. If you find, for example, a deviation of 10° pro-
duced by a medium of 1 inch in depth, a depth of 10 inches
would produce a deviation of 100°; so that by merely vary-
ing the thickness or depth of the medium, you may have the
position of greatest obscuration situated in *any* part of the
circle, and if the thickness happened to be such as that such
position coincided with 180° or 0°, you might suppose that
the medium was not possessed of circular polarization at all;
the importance, therefore, of examining different depths or
thicknesses will be apparent; and the depth of each medium

which produces a deviation of 180°, furnishes a very useful index of their comparative circularly polarizing energy.

If the depth be constant and two circularly polarizing liquids be mixed together, or if the fluid or substance be dissolved or mixed in some other fluid possessing no rotating energy, the deviation will bear a direct relation to the proportion in which the two are combined. For instance, since, according to my own experiments, 10 inches of a solution of cane-sugar in water containing half its weight of solid sugar produces a deviation of 104°, on mixing such solution with an equal bulk of water, 10 inches of such diluted solution would produce a rotation of only half that amount, or 52°, which coincides with experiment; having, therefore, once obtained the rotating power of a solution of given strength, we may, according to Biot, calculate thence the strength of any other solution when examined in a similar manner; attention, however, must be paid to the effects of temperature, any condensation which may take place on mixture, specific gravity, &c.; and I have not found the result of experiments quite accord with those of calculation, although the difference has been but slight, and this is probably due in some degree to the circumstances hereafter mentioned.

The rules of proportion do not directly apply to the percentage of sugar by weight contained in solution, unless the specific gravity and bulk be likewise taken into account. For instance, the solution referred to was prepared as follows:—

<div style="text-align:center">1000 grains dry sugar,
1000   ...   water,</div>

and its specific gravity was 1·2371; consequently 1 gallon of such solution would weigh 96,597 grains, and contain 48298·5 grains sugar. Now since 10 inches of such a solution produced a rotation of 104°, on adding an equal bulk, that is, another gallon of water to the original gallon, 10 inches would produce a rotation of 52°; but 1 gallon of water weighs 70,000 grains, which added to the weight of the syrup, 96,597 grains, gives the weight of 2 gallons, 166,597 grains, supposing no condensation to arise from the mixture; consequently 1 gallon of the new solution would weigh 83298·5 grains; and since it contains half the original quantity of sugar, or 24149·25 grains, the proportion by weight will be 24149·25 sugar to 59149·25 water, since 24149·25 + 59149·25 = 83298·5; so that the proportion of sugar by weight to the water is less than one-half, which would have been the case were the rotation in direct proportion to the weight of sugar as compared with that of the water in which it is dissolved, without reference to the bulk occupied by the mixture.

Again, if the proportions of the combination be constant, but the depth varied, the degrees of rotation will, as before stated, bear a direct proportion to the depth; thus, since 10 inches of a solution of cane-sugar, containing one-fourth its weight of solid sugar, produces, according to my experiments, a rotation of 50°, if the depth be 1 inch it would produce a rotation of 5°, and 36 inches would produce a rotation of 180°, and consequently with such a depth there would be no apparent rotation.

Having thus shown that neither the direction in which the analyser is rotated, nor the appearances presented at any given point, can alone enable us to determine whether the rotation is right or left-handed, we proceed to explain that such information can only be obtained by observing the order of succession in which the different tints or shades of obscuration present themselves.

Removing the plane of polarization, by revolving the polarizer, any number of degrees not exceeding 90° either to the right or to the left, it will be readily observed that the direction in which it has been moved might be ascertained by observing whether on moving the analyser the image X became darker or brighter; the image X becoming darker when the analyser is moved in the *same* direction as the other, but lighter if moved in an opposite direction. This law, however, it will be also seen is reversed if the rotation exceeds 90°. This method, then, affords the means of ascertaining the direction of rotation, *provided* it does not exceed 90°; the rotation being *right*-handed, if, when the analyser is *turned to the right-hand*, the image which is the subject of observation becomes darker and darker, and *left*-handed if it becomes lighter.

It matters not whether X or O be the subject of observation, or whether X be light and O dark when set to 0°, provided the amount of rotation be estimated from the position necessary to restore either ray to precisely the same condition in which it appeared at 0°, *previously* to the introduction of the circularly polarizing medium. When, however, a double refracting analyser is used, I would in all cases recommend the experiment to be commenced by placing the analyser and polarizer in such a position as that X is at its greatest point of obscuration when the vernier or index points to 0°. When a Nicol's prism or tourmaline is used as an analyser, presenting only one image, then, previous to the introduction of the circularly polarizing medium, it must be so placed with respect to the polarizer as that the index of the vernier points to 0° when the light is most completely polarized or obscured. Uniformity in the mode of conducting these experiments will greatly promote accurate results.

To return, however, to the means by which the direction of the rotation is to be ascertained. The method already described might be applied in examining solutions of weak rotating energy which exhibit little or no colour when examined by polarized light, *provided it is not confounded with the effects of mere depolarization*, the appearances then presented being similar in some respects to those produced by double refracting substances, as described in the twenty-first chapter of Brewster's Treatise on Optics. When substances of greater energy are examined, they exhibit a gradation of colours corresponding to those of the solar spectrum, which affords a still more satisfactory mode of determining whether the rotation is right- or left-handed. The direction corresponds to that in which the analyser must be moved in order to obtain the succession of colours as follows, viz. red, orange, yellow, green, blue, indigo, violet. If the analyser be moved in only one direction, and that right-handed, or in the order of the figures in the outer circle of graduation, then the rotation will be right- or left-handed, according to the succession of the colours in the following tables. The general order of succession when the *analyser* is moved in a *right*-handed direction is first given, viz.

Left-handed.			Right-handed.	
X.	O.		X.	O.
Violet	Yellow	0°	Violet	Yellow
to	to	to	to	to
Green	Orange	45°	Orange	Green
to	to	to	to	to
Yellow	Violet	90°	Yellow	Violet
to	to	to	to	to
Orange	Green	135°	Green	Orange
to	to	to	to	to
Violet	Yellow.	180°	Violet.	Yellow.

The following table gives a more detailed account of the succession of tints produced, and commences, it will be observed, with the boundary of the blue and violet, which is the darkest ray, corresponding to the greatest obscuration of X, and is the point at which the degree of rotation should be observed. We may derive much assistance in estimating its precise position, when a double refracting analyser is employed, by examining the appearance of O, which will then be at its maximum of illumination and of the brightest and purest yellow tint. When we travel too far into the violet with X, O will assume a greenish-yellow tint; when too far into the blue, the yellow will deepen towards an orange. If a tourmaline or coloured polarizer be employed, attention

must be had to its effects upon the yellow ray, which may be easily ascertained by observing the appearance of O previous to the introduction of the circularly polarizing medium.

Left-handed.			Right-handed	
X.	O.	°	X.	O.
Blue violet.	Bright yellow.	0	Blue violet.	Bright yellow.
Blue.	Deep yellow.	10	Violet.	Greenish yellow.
Greenish blue.	Orange yellow.	25	Crimson.	Pale green.
Green.	Orange.	35	Reddish orange.	Light green.
Light green.	Reddish orange.	50	Orange.	Green.
Pale green.	Crimson.	65	Orange yellow.	Greenish blue.
Greenish yellow.	Violet.	80	Deep yellow.	Blue.
Bright yellow.	Blue violet.	90	Bright yellow.	Blue violet.
Deep yellow.	Blue.	100	Greenish yellow.	Violet.
Orange yellow.	Greenish blue.	115	Pale green.	Crimson.
Orange.	Green.	125	Light green.	Reddish orange.
Reddish orange.	Light green.	140	Green.	Orange.
Crimson.	Pale green.	155	Greenish blue.	Orange yellow.
Violet.	Greenish yellow.	178	Blue.	Deep yellow.
Blue violet.	Bright yellow.	180	Blue violet.	Bright yellow.

The following general conclusions will be easily understood from the Tables.

That, whether the rotation be *right-* or *left*-handed, the *order* of succession of colours is the *same in* X as it is *in* O, the one lagging as it were 90° behind the other.

If the analyser be revolved so as to obtain what may be termed a *descending* succession of colours, viz. red, orange, yellow, green, blue, &c., the *direction* in which it is revolved *corresponds* to that of *rotation*.

If the analyser be revolved to the *right*-hand when examining a substance possessed of *left*-handed rotation, the order will be in what may be termed an *ascending* scale, viz. blue, green, yellow, orange, red.

If in examining a substance possessed of *right*-handed rotation the analyser be turned to the *left*, the order will also be in an *ascending* scale, viz. blue, green, yellow, orange, red, &c.

That, *whether* the rotation be *right* or *left*, provided the amount is alike, X and O will exhibit the *same tints* at a *similar number* of degrees of rotation marked in the right- or left-handed circles of graduation respectively.

Hence, at the points of greatest obscuration, there is *no* difference in the appearances exhibited, whether the rotation be *right-* or *left*-handed.

That X assumes the tint of O, and O that of X at an exact interval of 90°.

That with a double refracting analyser each particular tint

occurs four times in the course of a complete revolution of 360°, viz. twice in the ordinary, and twice in the extraordinary ray. With a tourmaline or a Nicol's prism, the tints will of course occur only twice in a whole revolution.

That in a revolution of 180° the tints have passed through all their phases.

The tints that have been described correspond to those of quartz, and are more or less vivid in proportion to the rotating energy. In fluids the blue violet generally assumes more of a neutral tint, and in many instances the crimson is scarcely to be observed. Of course, if the fluid be itself possessed of colour, it will greatly interfere with the purity of the tints. When using a piece of red glass, as recommended by Biot, to obtain homogeneous light, the tints disappear, and the obscuration of X, which is then more complete, will determine the amount of rotation. The red glass sent by Mons. Soleil with Biot's apparatus is much too dark, and occasions so great a loss of light as materially to interfere with the delicacy of our readings. Of course the *direction* of the rotation should be observed *without* it.

Daylight is preferable to artificial light for these experiments; unless, as before explained, in the case of a coloured polarizer, attention is paid to the appearance of O when artificial light is employed, the results will not agree with those obtained by daylight.

Having explained the appearances which are to guide our experiments, I proceed to a description of the apparatus which I constructed for the purpose, and which is similar in some respects to the arrangement of the Rev. B. Powell, described in the Philosophical Magazine for April 1843.

M N O P (figs. 1 and 4, p. 429) is a wooden box, the lid of which *b b* not only opens in front, but may be also fixed at any height, and fastened by a small bolt at *a*, fig. 1. This box serves the purpose of a darkened chamber, and the apparatus may be packed in it when not in use. K K is a moveable perforated shelf, which may be also secured at any height by means of the screw at I, fig. 1, passing through an opening in the back of the box shown at X, fig. 4. On this shelf the vessel containing the fluid to be examined is to be placed, its centrality being provided for by fitting into another loose shelf turned out to suit it. To the under side of the shelf K a brass plate is screwed, fitted with a socket to receive the polarizer G, which may be either a tourmaline or a Nicol's prism, and which may be revolved by means of the collar attached to it for that purpose. H is a mirror to reflect the light through the polarizer and liquid towards the analysing

double refracting eye-piece A B C D, fig. 3, and A, figs. 1, 2 and 4. This eye-piece consists of a lens C placed at its focal distance from a small aperture at D, so as to give a well-defined image of it when transmitted towards the observer's eye at A; B is a double refracting achromatic analyser of Iceland spar, or of quartz, which is placed within a tube which slides

Fig. 1.   Fig. 2.   Fig. 4.
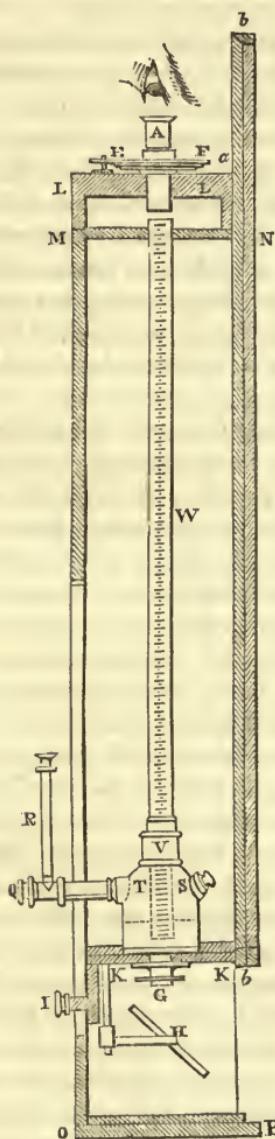
Fig. 3.
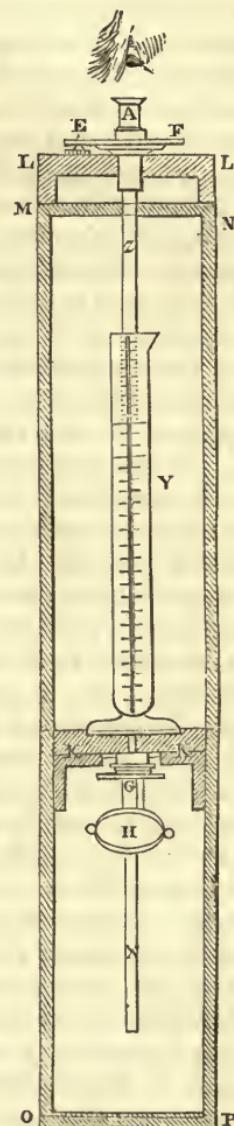
into the tube containing the lens, and also fits into the socket of the vernier, it being sometimes desirable not to use the lens. A Nicol's prism, or a tourmaline similarly fitted, may occasion-

ally be substituted. This analysing eye-piece fits tightly into a socket adapted to the vernier E of a graduated circle F, fig. 2. The analyser may be adjusted by turning it in its socket, and when set to its proper position revolves with the vernier. The graduated circle, with its vernier and eye-piece, is attached, for the convenience of removal in packing, to a loose shelf L L, and is always secured in the same position by steadying pins. The upper end of the box has a circular opening of about $1\frac{1}{2}$ inch diameter, which serves to centralize and steady the tube passing through it, as shown in fig. 1. S T V, fig. 1, is a three-necked glass vessel to contain the liquid to be examined. W is a tube graduated into inches and tenths, passing through an air-tight, brass stuffing box, cemented to the vessel at V. S is an opening fitted with a stopper, which is useful in filling, emptying and cleansing the vessel. R is a small condensing air syringe adapted to the vessel at I, and Q is a brass screw, for the purpose of allowing the air to escape when requisite. The method of using the apparatus is as follows:—First, secure the shelf at the proper height to receive the vessel to be subsequently introduced, then adjust the mirror to reflect the light, of the sky or a lamp, upwards towards the analyser at A, set the vernier to zero on the graduated circle, then place the analysing eye-piece in the vernier, so that the extraordinary image X is also directed towards zero; next revolve the polarizer G until X has attained its maximum of obscuration, taking care not to disturb the vernier or mirror. The apparatus is thus ready for experiment, and the liquid to be examined must next be introduced. It may either be contained within a glass vessel, S T V, such as we have already described, or in a simple graduated glass tube, as shown at Y, fig. 4, in which case the brass tube Z, blackened on the inside, and fitted on to the lower part of the analyser, is occasionally useful, and lessens the necessity of closing the door to obtain a darkened chamber. The bottom of either vessel should be very transparent and free from irregularity or specks. These vessels are to be placed upon the shelves, as shown in the drawings, and the door of the box closed. The vessel S T V is contrived for the purpose of examining different depths of the same liquid without removing the vessel, which is necessary when using only a simple tube, as shown at Y, fig. 4. By condensing air into the vessel by means of the small syringe, the liquid may be raised to any height in the tube W, and it may again be lowered by allowing the air to escape at the screw I. The liquid having been introduced, suppose within the vessel S T V, and occupying a depth, say of 2 inches, the image X, which was invisible before, will then,

in all probability*, become apparent and tinted, if not deci-
dedly so, raise the liquid by means of the syringe until it does,
and observe the depth ; then move the vernier in a right-
handed direction until the image X attains its maximum of
obscuration, and read off the amount of rotation ; continue
the movement of the vernier until O next becomes obscured,
then again observe X, and then again O. Thus you will ob-
tain four readings corresponding to the depth observed, each
of which should differ from the preceding 90° if your observa-
tions have all been correct, but if not, a mean must be taken;
and it will in all cases be better to take a mean of several ob-
servations, and again to compare such mean with that ob-
tained from an observation of the height necessary to rotate
the ray 180°, which is procured as follows :—Having placed
the vernier so that X is at its greatest point of obscuration,
and observed the height at which the liquid stands, pump in
air to raise the liquid until O becomes most obscured, and
observe the additional height necessary to effect this, corre-
sponding to a rotation of 90° ; continue the raising of the liquid
until X again becomes obscured, and again observe the addi-
tional height, which, if the experiment be correctly conducted,
should be equal to the former, and of course the total height
will correspond to a rotation of 180°. It is unnecessary to
say anything more about the mode in which the direction of
the rotation is to be ascertained, this having been already fully
explained. I shall conclude, therefore, with a few further ob-
servations on sources of error which may tend to mislead or
to interfere with the accuracy of observations. When exa-
mining different heights care must be taken that the fluid which
adheres to the sides of the tubes has time to subside, otherwise
the apparent amount of rotation will be thereby increased.

Great care must be taken that the position of the mirror
is not altered after the adjustments are made, as with some
Nicol's prisms I have found a very slight change of the incli-
nation of the mirror alter the plane of polarization altogether.

After the adjustments are made, any double refracting me-
dium placed upon the shelf *will cause the image X to become
depolarized and therefore luminous ; this must not be con-
founded with circular polarization*, which I suspect has been
the case. Many oils, and some solutions of grape and potato
sugar, exhibit this phænomenon. The difference from circular
polarization will be readily understood by attending to our
former instructions, and observing that in these cases there is
no alteration of the position of the plane of polarization exhi-
bited on rotating the analyser, more especially on increasing

* Alluding to the accidental circumstance of the depth corresponding to
180° rotation.

the depth of the liquid; I would strongly advise any one experimenting on this subject to examine the appearances presented by pieces of unannealed glass, crystals, mica, selenite, &c., by placing them upon the shelf K K when the apparatus is adjusted.   I should however observe, that some specimens of unannealed glass, mica and selenite, possess circular polarization as in quartz, and it probably exists to a much greater extent than has been hitherto noticed.

Water cooled below 39° is a curious example of the phænomenon of depolarization last alluded to, which I discovered about three years since, but did not then feel sufficiently satisfied with the mode of observation.   I have now completely verified the fact, which may be readily exhibited by means of the apparatus now described.   Since this paper was read I have however obtained apparent evidence of rotation, amounting to about 15 degrees for a depth of 15 inches, but rapidly disappearing as the temperature of the water rises.   I am about repeating these experiments, and shall communicate the result in a future paper[*].

When a Biot's apparatus, as constructed by Mons. Soleil, is used, mistakes may arise, not only from the confusion of the extraordinary and ordinary images revolving round each other, before alluded to, but also from the graduation of the circle, which is divided to 180° on either side of 0° and no further.   Supposing, for instance, a rotation of 79° to the right, since on moving the vernier 11° to the left the obscuration of O would be observed, it might be mistaken for a left-handed rotation of 11°.   Probably some such error occasioned Soubeiran to state that a solution of sugar which showed an original rotation of 71°, was altered after boiling as in the following table:—

Primitive syrup	.	.	.	.	71°	right-handed.
After 20 hours	.	.	.	.	0	
... 25 ...	.	.	.	.	11°	left-handed.
... 64 ...	.	.	.	.	0	
... 74 ...	.	.	.	.	5°	right-handed.

Supposing Soubeiran mistaken as to the direction of the rotation (and he tells us nothing of the succession of tints by which it might have been determined), but correct as to the points of obscuration, his statements would lead us to infer that it was really a right-handed rotation throughout, but increasing, as it naturally would do, from + 71° to + 95° as the water evaporated.   The difficulty of determining the exact

---

[*] I have since discovered that this apparent circular polarization was due to the effects of temperature upon the bottom of the glass vessel, a circumstance which materially interferes with the delicacy and certainty of such observations. I am still engaged in these experiments. Nov. 13, 1844.

reading, which requires much practice, will readily account for an error of a few degrees, and the subjoined table (based on the supposition that the original bulk was preserved as it ought to have been by the addition of as much water as was lost by evaporation) will show that in the last four experiments O_2 was probably mistaken for X.

Yellow ray, depth 100 *millimetres* = 3·937 *inches.*

	Soubeiran.	X.	O.	X_2.	O_2.
Primitive syrup	+71	71°	161°	251°	341° = − 19
after 20 hours	0	90°	180°	270°	360° = 0
... 25 ...	−11	79°	169°	259°	349° = − 11
... 64 ...	0	90°	180°	270°	360° = 0
... 74 ...	+ 5	95°	185°	275°	365° = + 5
		5)425°	5)875°	5)1325°	5)1775°
Means ...	.....	85°	175°	265°	355° or − 5

The above table will give a general idea of the mode in which the results are to be taken and tabulated. I find, as might be expected, that solutions of sugar become decomposed by keeping and by heat, but the effect is simply to diminish the amount of rotation, and not to alter the direction. I have already adverted to the care required in observing the precise point of obscuration, viz. the extreme boundary between the blue and violet ray, and to the assistance to be derived from observing the complementary tint.

It is desirable to preserve one position during the observations with the face towards zero, and to look steadily in a perpendicular direction, as the appreciation of the tint will vary somewhat if the position of the eye be altered*. I generally get an assistant to note the numbers, so that the observations may be made with as little looking off as possible. The effect of gazing at all coloured objects must be remembered, and it would be well to have the whole of the apparatus blackened, or, where convenient, to make the observations in a darkened chamber. The loss of light occurring from an increased depth or strength of liquid, especially if such liquid be coloured, must be also considered, and will frequently prevent the experiments made at different depths from according as they should do with each other. With constant practice I can obtain a reading to half a degree, beyond which I do not think it is possible in most cases to obtain any certainty ; and of course the method does not furnish an

* With some analysers the plane of polarization will be entirely changed if the position of the eye be greatly altered.

accurate substitute for chemical analysis, although it may be useful as a rough mode of appreciating the purity of an oil or the strength of a solution of sugar.

I subjoin a table of the results of some experiments, and shall have great pleasure in examining any other samples that may be sent or brought to me at the Laboratory at St. Thomas's Hospital.

Table of some rotating Liquids.
Right-handed.

	Depth 10 inches.	180° rotation.
Essential oil of caraway .	200°	9 inches.
... ... dill . . .	190°	9·5 ...
... ... lemon . .	150°	12 ...
Rectified oil of turpentine .	50°	36 ...
Common oil of turpentine .	40°	45 ...
Essential oil of nutmeg . .	33·5°	53·46 ...
Solution of 1734 grs. camphor in 2105 grs. alcohol	52°	34·5 ...

Solutions of cane-sugar in cold distilled water :-

Equal weights, sp. gr. 1·2371		104°	17·307...
1 sugar to 2 water	1·1473	70°	25·7 ...
1 sugar to 3 water	1·107	50°	36·0 ...

Left-handed.

	Depth 10 inches.	180° rotation.
Essential oil of cubebs . .	83°	21·8 inches.
... ... lavender .	20°	90 ...

Table of Essential Oils examined and possessing no rotating energy.

Essential oil of anise.
...   ...   cloves.
...   ...   pimento.
...   ...   chamomile.
...   ...   calamus.

LXXIII. *On the Cause of the Reduction of Metals when solutions of their salts are subjected to the Galvanic Current. By* ALFRED SMEE, *Esq.,* F.R.S., *Surgeon to the Bank of England, &c.*[*]

1. $\mathbf{A}$T the present time, when the new science of electro-metallurgy is improving and multiplying the arts of this already extensive manufacturing empire, there cannot be a subject more fit for the consideration of the Fellows of the

Royal Society than the cause of metals being reduced when solutions of their salts are subjected to the voltaic circuit.

2. The opinions of philosophers upon this point, from the period when electricity first lent its mighty aid to chemists, are various. Some have supposed that hydrogen evolved by the decomposition of water reduces the metals, others that the poles directly attract the metals to their surfaces, and lately a paper has been printed in the Transactions of this Society, whereby a new constitution of the salts is inferred; the acid and oxygen being supposed by electrolysis to pass in one direction, the metal in the other. The first opinion was put forward by Hisinger and Berzelius, and may be found in the *Annales de Chimie*, vol. li. p. 174. "Il résulte de tous ces faits, que l'on a une idée fausse de la réduction opérée par l'électricité, puis qu'on l'attribue au dégagement de l'hydrogène, comment expliqueroit-on la réduction du fer et du zinc, qui ont la propriété de décomposer l'eau sans électricité."

A similar opinion has been advocated by Faraday in the Philosophical Transactions, and he applied a new name to this kind of action, giving it the term electro-chemical action. The second hypothesis was promulgated by Sir Humphry Davy, who states, "that hydrogen, the alkaline substances, the metals, and certain metallic oxides are attracted by negatively electrified metallic surfaces and repelled by positively electrified metallic surfaces; and contrariwise, that oxygen and acid substances are attracted by positively electrified metallic surfaces, and these attractive and repulsive forces are sufficiently energetic to destroy or suspend the usual operation of chemical affinity." (Phil. Trans., 1807, p. 28.)

3. The hypothesis of the direct electrolysis of metallic salts has been advanced by Prof. Daniell in consequence of some ingenious experiments which have been detailed before this Society, and in which it is supposed that he directly stopped the metal in its passage to the negative pole. The mode in which the experiments were performed is as follows:—A solution of the metallic salt is placed on the positive side of a diaphragm apparatus and a solution of potassa on the other side, when, on the circuit being completed by a powerful battery, the metal is deposited on the diaphragm. From this experiment it has been conceived that the acid and oxygen are in combination, forming a proximate principle, which in sulphate of copper is called oxysulphion; and the salt of copper is believed to be an oxysulphion of copper. When this salt is subjected to the voltaic circuit he believes it to be directly electrolysed, the oxysulphion passing one way and the copper the other.

4. This experiment is so much at variance with the electro-chemical doctrine of Dr. Faraday and the inference to be drawn from my cinder experiment, that on rewriting my Elements of Electro-Metallurgy, I felt it to be my duty to examine carefully this hypothesis before I adopted or rejected such an important doctrine. For this purpose his experiments were repeated and varied in different ways; and with alkalies on the negative side I readily and immediately obtained the metal on the diaphragm, but failed when neutral salts were used on the negative side, or when solutions of gold and platinum were employed on the positive. A series of experiments were then instituted on the polarity of solid substances interposed between two platinum poles. I placed a series of copper wires in all directions and situations between the poles in a solution of sulphate of copper, and found that one part of each wire became positive and was dissolved, and another part of the same wire became negative and reduced the metal from the solution; so that all the wires were, at different parts, either dissolving or increasing. I next extended my examination as to the capabilities of platinum wires to become polar, and found that a much greater resistance was effected in this case than when other metals were employed; still, however, by particular management they were readily made to give off abundance of gas (Elements of Electro-Metallurgy, p. 53, 2nd edition).

5. The polarity of interposed conducting substances having been fully proved, I endeavoured to ascertain how far non-conducting bodies would take on a similar condition, but could not obtain very satisfactory proof on this matter: sufficient evidence not being obtained to prove the possibility of interposed non-conducting substances to become polar, I determined not to rely on the investigation for the cause of the reduction of the metals on these experiments, but seek proofs from other causes.

6. I repeated the experiments made in 1839 and communicated to the Philosophical Magazine in 1840, from which I discovered that porous coke or charcoal, when arranged as the negative pole in dilute sulphuric acid, absorbed a large quantity of hydrogen. The mode in which I repeated these experiments was as follows:—A piece of well-burnt charcoal or cinder was removed red-hot from the fire and quenched in dilute sulphuric acid, so that all chance of the presence of oxygen in its texture might be precluded. In this state it had no effect on metallic solutions. It was then arranged as the negative pole in dilute sulphuric acid by connecting it with the voltaic battery, when it became charged with hydrogen,

which was ascertained by the circuit being completed some time before gas was evolved from its surface. Pieces of coke thus charged with hydrogen were dipped into various metallic solutions, when the metal was instantly deposited as a brilliant coating on the coke. The deposit of copper affords the most beautiful example, though the deposit of other metals, as of silver and gold, is also striking. Coke charged with hydrogen retains the property of reducing metals many days after its first saturation.

7. The repetition of these experiments deeply impressed my mind that hydrogen evolved from the decomposition of water is really the cause of the reduction of the metals, and I pursued my experiments to test this idea, the first promulgated on the subject, and the one supported by our eminent galvanist, Dr. Faraday.

8. The next stage of investigation consisted in inquiring as to whether the transfer of the metal took place uniformly and synchronously with this reduction. It is apparent that such a transfer is absolutely necessary to the theory of the direct electrolysis of the metallic salt, but let us see what is the result. I prepared an uniform solution of sulphate of copper, and placed at the bottom of a tall vessel containing it a positive copper pole, and at the upper part a negative pole of the same metal, forming a circuit by means of one or two cells of my galvanic battery. On the action of the voltaic force bright reguline copper at first appeared at the negative pole; this was followed by a brittle, this by a sandy, this by a spongy deposit, this by black powder, and finally hydrogen was evolved.

9. This most important experiment was repeated in a variety of ways and with various metallic salts, with the details of which there is no need to trouble the Society, because the experiment just described as an example points out in the most positive manner that the transfer of metal is not proportionate to the metal reduced. The positive pole indicates the same fact, for it became coated with metallic salt, showing beyond all source of fallacy that the metallic salt was directly formed at one pole and the metal directly reduced from another portion of the metallic solution at the other. In a neutral solution of any metallic salt, as that of copper, the acid is no more equally transferred to the positive pole than the metal is to the negative; and we find in its decomposition that the positive pole is frequently coated with oxide of copper of considerable thickness, and from the liberation of an equivalent of sulphuric acid from the sulphate the solution speedily becomes acid.

10. Not only in the above-described experiments have I

observed this fact, but in the experiments of the decomposition of above one hundred different salts, including those of twenty distinct metals which I performed for my work on Electro-Metallurgy, did the results show themselves more or less evidently; and I may state that no electro-metallurgical process can be conducted without the production of these phænomena; and one of the greatest, if not the very greatest difficulty which the electro-metallurgist has practically to contend with, is the variation in the strength of the metallic salts subjected to decomposition*.

11. In what way are the metals reduced if they are not transferred from pole to pole? A careful investigation of a solution depositing its metal will at once explain the phænomenon, for from the negative surface a colourless lighter fluid is seen to rise, which in some cases will destroy the electro-metallurgist's hopes, by causing grooves on the reduced metal, sometimes even to the extent of an inch or more in depth, and the same fluid at other times will cause a sandy or spongy deposit. From these facts it is evident that water is formed at the negative surface, and that hydrogen probably reduces the metallic salt.

12. We have now as arguments in favour of the reduction of the metals by hydrogen gas liberated by decomposition of the water, first, the results obtained by the cinder experiment; secondly, the non-transference of the metal; thirdly, the imperfect transference of the acid; fourthly, the rise of a light aqueous fluid from the negative pole of a metallic solution. We have, fifthly, besides these cases, the analogy of the reduction of nitrous acid in Grove's battery by hydrogen, for the removal of the gas by the acid gives to it its intensity; and in this case no other mode can even be supposed by which the decomposition of the acid is effected, save and except its decomposition by hydrogen. The further alteration of the per-

---

* November 20, 1844.—I happened to call yesterday upon that excellent practical electro-metallurgist, Mr. Horne of Newgate Street, and found that he was making an electro-statue of Sir John Crosby, in a mould prepared by the sculptor Mr. Samuel Nixon, from the original model also executed by him. Mr. Horne was engaged about the leg, and he stated that he was much troubled by the non-uniform diffusion of the metallic salt through the solution. The mode in which he overcame the difficulty was very simple: he introduced a glass tube every now and then to the bottom of the leg and blew through it, which caused a proper mixture of the exhausted and saturated solutions. The fact was a pretty practical example of the opinions which I have been endeavouring to enforce; it shows that the theorist may in vain attempt to bolster up crude notions, for the practical man must, to be successful, not only adopt, but act upon the true rationale of the phænomena. Societies may determine that the metals are directly electrolysed, but the practical man will find that their edicts will have but very little effect upon his processes.—A.S.

salts of metals into protosalts favours strongly, by analogy, the same opinion. Such evidence is amply sufficient, but I have now to detail some direct experiments in which hydrogen alone reduces metals from various salts.

13. The mode in which this satisfactory result is obtained is very simple. The metallic solution to be examined is placed in a glass, a test-tube filled with the same solution is then procured, into which a slip of platinum, platinized as for the construction of my battery, is then placed. The whole is then placed within a glass of metallic solution, and a current of hydrogen introduced sufficient to extend half-way up the platinized platinum. It will be seen that the arrangement is similar to that of one of the tubes of Prof. Grove's elegant gas battery, and the various results obtained by this mode of examination it will be my duty immediately to explain.

14. If the solution employed be chloride of platinum the hydrogen disappears gradually, and the black powder of platinum is coated here and there by a beautiful bright layer of reguline metal.

15. With a solution of chloride of gold the same result is seen, the platinum being gilt with a beautiful deposit of metal. With this salt the action is immediate.

16. With nitrate of palladium the hydrogen is rapidly absorbed and bright palladium deposited.

17. With nitric acid and platinized platinum the hydrogen is gradually absorbed and the acid decomposed.

18. A solution of sulphate of copper under similar examination yields a plentiful crop of small crystals of copper in a short period.

19. A solution of persulphate of iron yields rapidly its oxygen and becomes converted into the protosulphate.

20. A solution of nitrate of silver yields slowly small crystals of silver, but the action with this metal is not so rapid.

21. The hydrogen was slowly absorbed when exposed to a solution of muriate of tin in contact with platinized platinum.

22. Nickel was slowly deposited from the chloride, when exposed under similar circumstances, as a white deposit.

23. Hydrogen, contrary to my expectations, did not appear to reduce iodine from iodic acid.

24. From the solution of protosalts of iron I could not determine that the metal was reduced.

25. I next subjected to experiment the extraordinary compounds of cyanogen forming the metallo-cyanides. From the aurocyanide of potassium I obtained metallic gold.

26. The argento-cyanide of potassium, in a similar way,

yielded slowly silver, the hydrogen becoming at the same time lessened.

27. These last two results are interesting to the chemist, for it appears to open a path for investigation on the nature of these very curious and now highly important combinations.

28. Such were my results with platinized platinum and hydrogen; the hydrogen becoming absorbed and the metals being reduced.   It was found that chloride of platinum, chloride of palladium, persalts of iron, chloride of gold, and nitric acid yielded the oxygen most rapidly.   It next became my endeavour to ascertain how far other metals, or different conditions of the same metal, might be used; and smooth platinum was next the subject of experiment.   With sulphate of copper or persalts of iron no absorption however took place.

29. Palladinized platinum was made the subject of experiment; and when employed with hydrogen in a manner similar to platinized platinum in sulphate of copper, it caused that metal to be reduced.

30. The effects of silver were then examined, and for that purpose a piece, carefully cleaned by nitric acid, was immersed in a solution of copper in contact with the hydrogen, but no change occurred even after many days.

31. No better result attended the employment of silver sponge used for the same purpose.

32. Nor did the black deposit of silver answer better to effect the absorption of the hydrogen, and the reduction of the metal from a solution of sulphate of copper did not occur.

33. Copper, both in its bright and spongy states, was next examined; in its bright state, cleansed by nitric acid, no change occurred.

34. In the spongy state it appeared to be of no greater efficacy to cause the reduction of the metal.

35. Considering the interest attached to carbon in the cinder experiment, detailed in a former part of this paper, a piece of carbon was exposed to the action of hydrogen and a solution of copper, but, singular to state, no deposit of copper took place, nor was any hydrogen absorbed.   In fact I have no mechanical method of charging coke with hydrogen, by which it can be made to cause the reduction of the metals.

36. The last set of experiments which I instituted upon the reduction of the metals by hydrogen was the value of bibulous paper to effect that object.   In dilute chloride of gold apparently no action after two or three days took place.

37. In a solution of sulphate of copper, after many days, no action apparently occurred with paper and hydrogen.

38. In a solution of nitrate of silver the same negative result with paper occurred*.

39. The persulphate of iron was not changed into the protosalt by the action of that gas in contact with paper.

40. As a summary of results obtained on the direct reduction of the metals from their solutions by hydrogen, it may be stated generally, that platinized and palladinized platinum alone were found to be competent to promote that change, negative results being obtained with smooth platinum, smooth silver, black powder of silver, bright copper, spongy copper, cinder or paper. These negative results are by no means to be taken as a proof that under these circumstances hydrogen might not reduce the metals, for it is possible under certain conditions that many other substances, like finely-divided platinum, might possibly take on the power of assisting in this mysterious way the absorption of the gas.

41. These various direct experiments of the deoxidizing agency of hydrogen gas are valuable to the electrician, as they point out the cause of some of the most interesting galvanic phænomena. To the electro-metallurgist they assume a much higher importance, for they point out to him, that in the decomposition of the metallic fluid the uniform strength will not be preserved by the changes taking place by virtue of the voltaic current. He must therefore remember in all cases to bring into play the attraction of gravity, not only to cause the diffusion of the newly-formed salt, from which the metal is to be deposited, but the acid to dissolve the oxide formed at the positive pole. The mode in which the metals are reduced may appear quite immaterial to the practical man, but he will find that the non-uniform diffusion of the metallic salt arising from the particular manner in which the reduction takes place, is the cause nearly of all his troubles.

42. From all these experiments we have an overwhelming body of evidence, of different kinds, to support the notion of the reduction of metals from their solution by hydrogen. The direct reduction in the cinder experiment, the non-transference of the metal, the imperfect transference of the acid, the rise of a light aqueous fluid from the negative pole, the analogous case

* November 4, 1844.—After some time the paper exhibited a black deposition of silver; the surface of the glass tube became incrusted with a brilliant layer of metallic silver. The importance of this experiment is manifest, for it shows that hydrogen and siliceous matter may at any time reduce metals from their solutions, a power doubtless in frequent operation in the bowels of the earth. How far metallic veins are thus deposited remains to be proved by future investigations. The decomposition of organic matter will always yield hydrogen, and hydrogen, under certain circumstances, will always reduce metals from the solutions of their salts.

of the decomposition of nitric acid and persalts of iron, and finally the direct experiments of the reduction of gold, silver, platinum, palladium, nickel, copper, tin, and the decomposition of the persalts of iron and nitric acid inclosed within a tube, appear to set the question to rest, and to point out in the clearest manner that hydrogen is the cause of the reduction of the metals. All the experiments which I have detailed only add confirmation to the valuable researches of Faraday on Electro-Chemical Decomposition, published in the Philosophical Transactions. To the inquiring mind a question naturally arises as to whether the hydrogen reduces the metal directly from the metallic solution, or whether it reduces its oxide. The former opinion, from the above experiments, appears to be most worthy of credit, though should other facts be discovered to elucidate that action, they will form subjects hereafter of a separate communication to this Society. It follows from these interesting experiments, that when a solution of metallic salt is subjected to the voltaic influence, the water is decomposed, oxygen passing one way and hydrogen the other; and that this hydrogen at the moment of decomposition on the negative plate performs the same part to sulphate of copper and other metallic salts that a piece of iron or zinc would to the same solutions.

LXXIV. *On the Existence of Absolute Criteria for determining the Roots of Numerical Equations. By* J. J. SYLVESTER, *Esq., M.A., F.R.S.**

I WISH to indicate in this brief notice a fact which I believe has escaped observation hitherto, that there exist, certainly in some cases, and probably in all, infallible criteria for determining whether a given equation has all its roots rational or not.

In the equation of the second degree it is enough, in order that this may be the case, that the expression for the square of the difference of the roots shall be a perfect square; in other words, if $x^2 - px + q = 0$ have its roots *rational*, $p^2 - 4q$ must be not only a *positive* number (the condition of the roots being real), but that number must also be a complete square. In this case it is further evident that (p) must be either prime to q, or if not, the greatest common measure of p^2 and q must be a perfect square; but this condition is contained in the former, which is a sufficient criterion in itself.

If we now consider the equation of the third degree,

$$x^3 - px^2 + qx - r = 0,$$

* Communicated by the Author.

one condition is, that the product of the squared differences shall be a perfect square; in other words, the equation cannot have all its roots rational unless

$$p^2 q^2 - 4 q^3 - 18 p q r - 4 p^3 r - 27 r^2$$

be a positive square number.

This remark is made at the end of the second supplement of Legendre's Theory of Numbers, and is indeed self-evident; and in like manner one condition may be obtained for an equation of any degree which is to have all its roots rational; but this is far from being the sole condition required.

In the equation of the third degree, however, one other condition, conjoined with that above expressed, will serve to determine infallibly whether all the roots are rational or not.

To obtain this condition, let us suppose that by making $3 x = y + p$ we obtain the equation

$$y^3 - Q x - R = 0.$$

Calling the three roots of this new equation α, β, γ (all of which it is evident must be rational if those of the first equation are so), we have

$$\alpha + \beta + \gamma = 0,$$
$$Q = - (\alpha\beta + \alpha\gamma + \beta\gamma) = \alpha^2 + \alpha\beta + \beta^2,$$
$$R = \alpha\beta\gamma.$$

From the last two equations it is easily seen that if k be any prime factor common to Q and R, k^2 will be contained in Q, and k^3 in R; or, in other words, k will be a common measure of α, β, γ.

We have therefore a *second condition*, that $9 q - 3 p^2$ shall be a negative quantity, which is either prime to $2 p^3 - 9 q p + 27 r$, or else so related to it, that the greatest common measure of the cube of the first and the square of the second is a perfect sixth power.

I now proceed to show the converse, that if these two conditions be both satisfied (and it will appear in the course of the inquiry that the first does *not* involve the second), the roots cannot help being all rational.

It is evident that the two conditions in question are tantamount to supposing that the roots of the proposed equation are linearly connected with those of another $z^3 - Q z - R = 0$ (by virtue of the assumption $3 x = k z + p$), where Q may be considered as *prime* to R; and where $4 Q^3 - 27 R^2$ is a perfect square.

Let now $4 Q^3 - 27 R^2 = D^2$, then $D^2 + 27 R^2 = 4 Q^3$, or $D^2 + 3 (3 R)^2 = 4 Q^3$.

Here, as Q is prime to R, D can have no common measure but 3, with 3 R.

1°. Let Q be prime to 3 R.

Then putting $f^2 + 3 g^2 = Q^3$, the complete solution of the equation immediately preceding is contained in the two systems.

$$\text{1st.}\quad D = 2f\quad 3 R = 2g.$$
$$\text{2nd.}\quad D = (f \pm 3g)\quad 3 R = f \mp g,$$

and for both systems,

$$f \pm g \sqrt{-3} = (h \pm 3k\sqrt{-3})^3.$$

The second system must therefore be rejected, for g evidently contains 3, and therefore $f = 3 R \pm g$ will contain 3, and therefore D and therefore Q will do the same, contrary to supposition.

Hence

$$\sqrt[3]{\frac{R}{2} \pm \sqrt{-\left(\frac{Q^3}{27} - \frac{R^2}{4}\right)}},$$

$$= \sqrt[3]{\frac{R}{2} \pm \frac{D}{2} : \sqrt{-\frac{1}{27}}},$$

$$= \sqrt[3]{\frac{g}{3} \pm f \sqrt{-\frac{1}{27}}},$$

$$= \mp \frac{1}{3\sqrt{-3}} \sqrt[3]{f \pm g \sqrt{-3}}$$

$$= - K \pm \frac{h}{3\sqrt{-3}} = \lambda \pm \mu \sqrt{-3};$$

and the three roots of the equation being

$$\begin{cases} (\lambda + \mu \sqrt{-3}) + (\lambda - \mu \sqrt{-3}) \\ \dfrac{1 \pm \sqrt{-3}}{2}(\lambda + \mu \sqrt{-3}) + \dfrac{1 \mp \sqrt{-3}}{2}(\lambda - \mu \sqrt{-3}) \end{cases}$$

will evidently be all rational, which of course includes the necessity of their being also integer.

Again 2°, if we suppose that Q does contain 3, D^2 will contain 27, and consequently D will contain 9; and we shall have

$$R^2 + 3 \cdot \left(\frac{D}{9}\right)^2 = 4 \cdot \left(\frac{Q}{3}\right)^3.$$

Here R being prime to $\dfrac{D}{9}$, it may be shown, as in the last case, that the complete solution is

$$\frac{R}{2} \pm \frac{D}{18} \sqrt{-3} = (h \pm k \sqrt{-3})^3,$$

consequently

$$\sqrt[3]{\frac{R}{2} \pm \sqrt{\frac{R^2}{4} - \frac{Q^3}{27}}} = h \pm k \sqrt{-3};$$

and the three roots of the equation are

$$2h, \quad h-3k, \quad h+3k \text{ respectively,}$$

and are therefore all rational.

Here it may be observed that the condition of R being an even number, which we know, *à priori*, is the case when all the roots are rational, is involved in the two more general conditions already expressed. It will now be evident that the first condition by no means involves the second, as it is perfectly easy to satisfy the equation $f^2 + 3g^2 = Q^3$ without supposing anything relative to k, the common measure of f, g, Q, except that it be itself of the form $\lambda^2 + 3\mu^2$, which will give

$$\left(\frac{f}{k}\right)^2 + 3.\left(\frac{g}{k}\right)^2 = (\lambda^2 + 3\mu^2)(r^2 + 3s^2)^3,$$

an equation which can be solved in rational terms for all values of λ, μ, r, s; and consequently the product of the squares of the differences of the roots may be a square, and at the same time the roots themselves may be irrational [*].

I believe it will be found on inquiry that the equation $x^n - qx + r = 0$ will always have two *rational* roots if

$$(n-1)^{n-1} \cdot q^n - n^n \cdot r^{n-1}$$

be a complete square, provided that q be prime to r.

Furthermore, viewing the striking analogy of the general nature of the conditions of rationality already obtained, to those which serve to determine the reality of the roots of equations, I am strongly of opinion that a theorem remains to be discovered, which will enable us to pronounce on the existence of integer, as Sturm's theorem on that of possible roots of a complete equation of any degree: the analogy of the two cases fails however in this respect, that while imaginary roots enter an equation in pairs, irrational roots are limited to entering in groups, each containing *two* or MORE.

4 Park Street, Grosvenor Square,
    November 7, 1844.

LXXV. *On some Photographic Phænomena.*
By GEORGE SHAW, *Esq.*[†]

IT is well known that the impression produced by light on a plate of silver, rendered sensitive by M. Daguerre's process, is wholly destroyed by a momentary exposure of the

---

[*] Thus then it appears that the *total* rationality of the roots of the equation $x^3 - qx - r = 0$ may be determined by a direct method without having recourse to the 0 method of divisors to determine the roots themselves; the two conditions being that $49^3 - 27r^2$ shall be a perfect *square*, and the greatest common measure of q^3 and r^2 a perfect *sixth* power.

[†] Communicated by the Author.

plate to the vapour of either iodine or bromine. Although this fact has long been known, the nature of the action by which so extraordinary an effect is produced, has not yet been satisfactorily explained. In the hope of elucidating this subject, a series of experiments was instituted, the results of which are recorded in the following remarks.

In order to prevent misapprehension, as well as to avoid circumlocution, it will be well, before proceeding to the experiments, to describe very briefly the various stages of the process by which the sensitive surfaces employed, were prepared. A smooth surface of pure silver plated upon copper is rendered perfectly clean, and is at the same time highly polished by being rubbed or scoured with a mixture of powdered tripoli and dilute nitric acid; the mixture being applied to and rubbed about the plate by a pledget of cotton wool. When the plate has been sufficiently treated in this way (the time required for which can only be determined by experience), the mixture is removed by pledgets of dry wool. The polish of the surface is still further improved by buffing the plate with a buff of clean cotton velvet on which fine charcoal powder has been sprinkled. The surface of a plate of silver thus treated is highly polished, and is assumed to be perfectly free from foreign matter. In order to render such a plate sensitive to light, it is supported over a vessel containing iodine, the silver surface being of course downwards. The vapour of iodine immediately attacks the surface of the silver, and forms, by combining therewith, a stratum of iodide of silver. By removing the plate from time to time and examining it by a feeble light, the relative thickness of the film of iodide of silver may be ascertained by its colour. The plate at first has a yellow colour, which by further exposure passes to a deep gold, to an orange, and next to a purple or rose colour; by still further exposure it becomes blue, and passes through a series of other colours; but as the purple or rose colour indicates that thickness of the film of iodide of silver most favourable to photographic experiments, it will be unnecessary to follow the plate through the changes consequent upon further exposure. This preparation of the silver plate is performed in a room from which daylight is excluded, or into which it is only partially admitted, for the purpose of examining the colour of the plate.

The film of iodide of silver thus procured is very sensitive to light, but may be made more so by exposure to the vapour of either chlorine or bromine. The quantity of either of these vapours necessary to communicate the maximum sensitiveness to the iodized plate is exceedingly small; and the pre-

sence of a larger quantity produces a state of insensibility to light. In order to determine the absorption of the proper quantity of the vapour of bromine (for bromine, from its superiority, is now used to the exclusion of chlorine), a very dilute solution of bromine in water is introduced into a glass vessel, and the iodized plate is suspended over it for a time varying according to the strength of the solution and the temperature, and which can be determined only by experiment. Instead of the treatment described, other processes may be resorted to for the preparation of the plate; a solution of chloride of iodine alone communicates sensitiveness to a silver plate, without a previous exposure to iodine; and instead of pure chlorine or bromine, the chloride of bromine, or bromide of iodine, may be used after the iodine. These, and various other accelerating substances, have been proposed, and are used by different experimenters; but they all contain chlorine or bromine, or both, and differ from each other only in the ingredients in which they (the active agents) are disguised. In the experiments hereafter related, it must be borne in mind, that although iodine and bromine are alone mentioned, and were for the most part used, yet the same results have been obtained by the use of all the accelerating agents enumerated, as well as several others.

A silver plate prepared by the process described, may be exposed to the vapour of mercury without being in any way affected by the exposure. If however the prepared plate be previously exposed to light, or made to receive the luminous image formed in the *camera obscura*, the mercurial vapour attacks it; forming, in the former case, a white film, and in the latter, a picture corresponding to the luminous image which had been allowed to fall on it.

If a prepared plate after receiving a vertical impression by light be exposed to the vapour of iodine or bromine, it is found that the vapour of mercury no longer attacks it; or, in other words, the impression produced by light is destroyed.

The first experiments made for the purpose of arriving at the cause of this phænomenon, had reference to the relation between the time of exposure to light and the time of exposure to the vapour of iodine or bromine necessary to destroy the effect produced by light. Prepared plates were exposed in the *camera obscura* for a length of time, which previous experiment had determined to be sufficient for a full development of the picture; some of those plates were exposed during two seconds to an atmosphere feebly charged with the vapour of bromine, while others were carefully preserved from contact with the vapours of iodine or bromine. The atmosphere

of bromine employed was produced by adding thirty drops of a saturated solution of bromine in water to an ounce of water; the solution was poured into a glass vessel, and the plate was exposed to the vapour in this vessel during the time specified. The plates were then introduced into the mercury box, and by volatilizing metal, the pictures were developed on all those which had not been exposed to the vapour of bromine, while those which had been exposed to it exhibited no trace of a picture under the action of mercury.

The same experiments were repeated with iodine, with ex-actly similar results.

Prepared plates were exposed to diffused light in the shade, and others were exposed to the direct rays of the sun; the object being in both cases the production of a more intense impression than that produced by the feeble light of the *camera obscura*. Some of these plates were exposed to the va-pour of bromine, and others to the vapour of iodine, while others were carefully preserved from the vapours of these substances. On subsequent exposure to the vapour of mer-cury, those plates which had not been exposed to iodine or bromine exhibited, by the large quantity of mercury which condensed on them, the effects of exposure to intense light; while those which had been subjected to the action of either bromine or iodine, were in no way affected by the vapour of mercury. Many repetitions of these experiments demon-strated that the effect of exposure to the most intense light was completely destroyed by the shortest exposure to the vapour of bromine or iodine.

Experiments were now instituted for the purpose of ascer-taining in what condition the prepared plate was left after having been first exposed to light and afterwards exposed to the vapour of bromine or iodine. In these experiments a method of treatment somewhat different from and more con-venient than that described, was resorted to, as in practising that method effects occasionally presented themselves which interfered with the results, and rendered it difficult to deter-mine with certainty how far some of the appearances pro-duced were due to the action of light. It has already been stated, that a prepared plate has a maximum sensitiveness when the iodine and bromine are in a certain relation to each other; if there be a deficiency of bromine, the maximum sen-sitiveness is not obtained, and, if there be an excess, the plate is no longer sensitive to light; but when exposed to the va-pour of mercury, *without having been exposed to light,* becomes white all over, by the condensation of mercury thereon; that is to say, it exhibits the appearance of a plate which had

been *properly* prepared, and which *had* been exposed to light.
From this it will be evident, that a plate properly prepared
in the first instance, and then exposed to light, may, by sub-
sequent exposure to the vapour of bromine, have the impres-
sion produced by light *wholly destroyed*; and yet, by the ac-
cumulation of bromine may exhibit, on exposure to mercury,
an appearance similar to that due to light. In other words, it
is impossible (in the case supposed) to distinguish between an
effect produced by light and an effect due to excess of bromine.
By using iodine in place of bromine, there is no risk of pro-
ducing the appearance which accompanies excess of bromine;
but on the other hand, by augmenting the quantity of iodine,
the sensitiveness of the plate is diminished. These difficulties
were overcome by using a solution containing both iodine and
bromine, in such proportions that the evaporation of each should
take place in the proportion in which they produce on silver
the most sensitive surface. The solution employed was made
by adding alcoholic solution of iodine to a solution of chlorate
of potash until the latter would take up no more of the for-
mer, and to each ounce, by measure, of this solution ten
drops of a saturated solution of bromine in water were added.
The solution of chlorate of potash was made by diluting one
part of a saturated solution of the salt with ten parts of water.
The use of the chlorate is simply as a solvent of iodine. In
the subsequent experiments the plate was exposed to the
vapour of this mixture of iodine and bromine with precisely
the same effect as when either was used separately, and with-
out the inconvenience or uncertainty which attended their
use.

A number of preliminary experiments, the detail of which
would be uninteresting, appeared to indicate, that not only
is the effect of light on a Daguerreotype plate destroyed by
iodine or bromine, but that the plate is restored to its original
condition; in other words, that its sensitiveness to light is
restored. In order to determine this point, the following ex-
periments were made.

A prepared plate was exposed to light, and afterwards to
the mixed vapour*; mercurial vapour produced no effect
upon it after a long exposure; the plate on removal from the
mercury box was a second time exposed to light, and again
introduced into mercurial vapour. The appearance of the
plate was very little changed, and it was concluded that no
effect, or, if any, very little, was produced by the second ex-

---

* I shall hereafter call the mixed vapours of iodine and bromine pro-
duced in the way described in the last paragraph but one, *mixed vapour*,
in order to avoid circumlocution.—G. S.

posure to light.   This conclusion was, however, erroneous, as
the following experiments prove.

A prepared plate was exposed to light, and afterwards to
the mixed vapour; mercurial vapour was found to have
no effect upon it; the plate was then partly covered with a
metallic screen, fixed close to but not in contact with it, and
the whole was exposed to light.   On placing the plate in the
mercury box, a broad white band, nearly corresponding to
the edge of the defended part, made its appearance ; the whole
of the defended part (excepting the band in question) was un-
affected, and the exposed part exhibited very little change.
By a careful examination of the plate after it was removed
from the mercury box, the white band in the middle appeared
to be produced by the feeble light which had passed under
the edge of the metal·plate which had screened the light from
part of the prepared surface; and the very dark, and appa-
rently unaltered appearance of the exposed part, was occa-
sioned by an excess of action, for mercury was found to have
condensed on that part in large quantity, and to have pro-
duced the dark lead colour which is commonly called *solari-
zation*; but which effect, in the case in question, was so ex-
cessive, that the colour of the part on which mercury had
condensed differed but very slightly from that on which no
light had fallen.   It was now evident that the apparent ab-
sence of effect in the last experiment was in reality occasioned
by an excess of action ; and by repeating that experiment,
and making the time of the second exposure to light much
shorter than before, the plate assumed, under the action of
mercury, an intense and beautiful whiteness.

From these experiments, then, it was perfectly clear that
the impression produced by light on a Daguerreotype plate is
wholly destroyed by the mixed vapour, and that *its sensitive-
ness to light is restored.*

It now remained to discover to what extent the sensitive-
ness is restored by the treatment in question.   It was not at
first expected that the sensitiveness to light was as great after
this treatment as after the original preparation of the plate ;
but experiment afterwards proved that the surface lost none
of its sensitiveness by this treatment, nor even by numerous
repetitions of it.   A prepared plate was exposed to light; the
impression was destroyed and sensitiveness restored by the
mixed vapour ; the plate was a second time exposed to
light and a second time to bromine, still its sensitiveness ap-
peared unimpaired; for a fourth or fifth exposure gave, on
treatment with mercurial vapour, a vivid impression.   In order
to determine with the greatest accuracy if the sensitiveness

of the prepared surface was at all impaired by these repeated exposures to light, the *camera obscura* was resorted to. A series of plates was prepared with the utmost attention to uniformity ; some of these were exposed in the *camera obscura,* and pictures obtained by the subsequent exposure to vapour of mercury ; the time requisite for the proper development of the picture was noted ; others were first exposed to the direct rays of the sun, and afterwards to the mixed vapour, and these were exposed in the *camera obscura* for the same length of time as those which had not been exposed to light. On treatment with mercurial vapour perfect pictures were produced, which could not be distinguished from those taken on plates prepared by the ordinary method. So completely does the mixed vapour restore the sensitiveness of prepared plates after exposure to light, that the most beautiful impressions were obtained in the *camera obscura* in two seconds on plates which had previously been *four* times exposed to the direct light of the sun, and after each such exposure treated with the mixed vapour.

As the plates experimented on, to this stage of the inquiry, had been *wholly* exposed to the sun's light previous to exposure in the *camera obscura,* it was thought that possibly some slight effect was produced, which, from being the same on all parts of the plates, escaped observation ; and in order to avoid the possibility of error from this cause, the impressions of light which it was intended to destroy by bromine were afterwards made in the *camera obscura.* Prepared plates were impressed with virtual images of different kinds, the *camera obscura* being pointed first at a house, afterwards to a bust, next to a tree, and finally to a living figure, the plates after each impression, excepting the last, being momentarily exposed to the mixed vapour. In every instance the most perfect impressions of the objects to which the *camera obscura* was last directed were obtained, and no trace of the previous impressions was left.

Experiments were next instituted for the purpose of ascertaining if the prepared surface, *after* the process of mercurialization, could be made to receive another impression by treatment with mixed vapour. Impressions were taken with the *camera obscura,* and after the full development of the picture by vapour of mercury, the plates were exposed to bromine and again placed in the *camera obscura,* the instrument being directed in the different experiments to different objects ; on exposure to mercurial vapour other pictures made their appearance, and although confused from superposition on the first pictures, could be clearly traced, and were found

perfect in every part. This production of picture upon pic-
ture was repeated, until by the confusion of the superposed
images the effects of further exposure could be no longer di-
stinguished.

In all the experiments hitherto described the destruction of
the impressions by bromine was effected in the dark, the ap-
paratus being situated in a room into which only a very feeble
daylight was admitted. It remained to be discovered if the
mixed vapour had the power of destroying the effect of
light while the plate was still exposed to light, or if the
vapour had the power of *suspending* or *preventing* the ac-
tion of light on a Daguerreotype plate. In order to deter-
mine this point, the apparatus was placed near the window of
a well-lighted room, and so arranged, that during the whole
time of the preparation of the plate, by exposure first to iodine
and afterwards to bromine, it was exposed to full daylight,
and by a mechanical arrangement, of too obvious a nature to
render description necessary, the plate was withdrawn from
the bromine vessel into a dark box ; that is to say, it was
withdrawn at the *same moment* from the influence both of light
and bromine : on being placed in the *camera obscura*, plates
so prepared received impressions, which by mercurialization
produced excellent pictures, and there was no trace of the
action of any light save that of the *camera obscura*. It follows
then that light is incapable of exerting any appreciable influ-
ence on Daguerreotype plates during the time they are re-
ceiving their coatings of iodine and bromine.

Although these experiments afford no information on the
subject in reference to which they were originally undertaken,
they are yet not without interest, both in their theoretical
bearing and in their practical application. They demonstrate
not only that the change (whatever it may be) effected by light
on silver plates prepared by Daguerre's process is completely
suspended in the presence of the vapour of either iodine or
bromine, but that after that change has been produced the im-
pression may be destroyed and the plate restored to its original
condition by a momentary exposure to either of these vapours.
In their practical application these experiments show, that all
the care which has been taken to exclude light from Daguerreo-
type plates during their preparation is unnecessary ; that so
far from a dark room being essential to the operations of the
Daguerreotype artist, the light of day may be allowed to fall
on the plate during the whole time of its preparation ; and
that it is only necessary to withdraw it at the same moment from
the action of bromine and light by sliding it from the bromine
vessel into the dark box in which it is carried to the *camera*

obscura; and where, from situation or otherwise, there is a difficulty in observing the colour of the plate during the process of iodizing, it may be removed from the iodine vessel and its colour examined by the direct light of the sun without risk of injury; for when returned to the iodine or bromine vessel for a moment the effect of light is wholly destroyed.

Perhaps the most valuable practical application of these facts is in the use of the same plate for receiving several impressions. When, on taking a portrait or the picture of any object liable to move, there is reason to suppose that the motion of the person or object has rendered the operation useless, it is not necessary to throw aside the plate on which the imperfect impression has been taken, and resort to the tedious process of cleaning and preparing another; it is only necessary to treat the plate in the manner already pointed out, and it is again equal in every respect to a newly prepared plate; and this treatment may be repeated until by the slow accumulation of too thick a film of iodide of silver, the plate no longer possesses the same degree of sensitiveness to light.

Temple Row West, Birmingham,   George Shaw.
    November 15, 1844.

LXXVI. *On the Constitution of the Urine in Man and Carnivorous Animals. By* Justus Liebig, *M.D., Ph.D., F.R.S., M.R.I.A., Professor of Chemistry in the University of Giessen*[*].

I T is a very curious and remarkable fact that all the analyses and examinations hitherto made of the urine of man and of carnivorous animals have not yet afforded us any satisfactory answer to the question, What is the substance which imparts to that fluid its property of reddening blue vegetable colours?

In most physiological and chemical works we find the acid reaction of urine ascribed either to uric acid or to lactic acid; but no positive and definite proof is given of the presence of the latter acid in urine.

The acidification of milk, that is, the formation of lactic acid, is dependent upon the milk-sugar contained in milk; this substance being in contact with caseine in a state of decomposition and transformation, undergoes, by its means, an alteration, which consists in its elements transposing and arranging themselves into lactic acid without the addition or separation of any one atom. Crystallized milk-sugar ($C_{12}H_{12}O_{12}$) and hydrate of lactic acid ($C_6H_6O_6$) have one and the same

[*] From the Lancet, Nos. xi. and xii., by the obliging permission of T. Wakley, Esq., M.P.

composition. It is a well-known fact, that when all the milk-sugar in a given quantity of milk has become converted into lactic acid, a fresh quantity of lactic acid may be produced by repeated addition of milk-sugar; indeed, the common white caseine in a state of decomposition yields, upon renewed additions of milk-sugar, almost unlimited quantities of lactic acid, provided the precaution be taken to neutralise the free acid from time to time by means of an alkali. Grape-sugar and cane-sugar, under the same conditions, yield the same products as milk-sugar. The examinations and experiments of Haidlen* have proved that milk in its fresh state contains neither lactic acid nor lactates.

So long as chemists were ignorant of the intimate relation existing between lactic acid and the milk-sugar contained in milk, there appeared to be some reasons for believing in the presence of lactic acid in animal secretions generally, when manifesting an acid reaction, or passing into a state of acidity. It thus happened, without any real and positive proof, that lactic acid was considered to be a constituent of urine ; the presence of lactates in the blood was assumed, and these salts were even supposed to perform a highly important part in the process of respiration.

Lactic acid is a non-nitrogenous substance. Nothing has hitherto been observed tending to show that it may be produced from the elements of a nitrogenous substance, by the decomposition of such a substance and the transposition of its elements. In every instance where the formation of lactic acid has been observed, the result of careful examination has proved the presence of a non-nitrogenous substance of an identical or, at least, similar composition with lactic acid.

These observations would seem to render the formation of lactic acid in the body of the herbivorous and graminivorous animals, which take starch and sugar in their food (substances from which lactic acid may be formed), not merely possible, but in many cases highly probable ; and yet, strange to say, chemists have hitherto attempted in vain to detect lactic acid in the urine of the cow and of the horse. The urine of the cow or horse has no acid reaction ; on the contrary, its reaction is strongly alkaline ; it contains carbonated, hippurated, or benzöated alkali, or alkalies combined with mineral acids, but no trace of any *lactate*.

In contrast with this, the urine of man, and of carnivorous animals, manifests, when in a healthy state, a strongly acid reaction. Now, it is precisely in analyses of the blood and urine of man, and of carnivorous animals, that we find lac-

* *Annalen der Chemie und Pharmacie*, vol. xi. p. 263.

tates mentioned as constant constituents; not because they have in reality been detected in these fluids,—for no one has as yet succeeded in producing lactic acid therefrom,—but because, upon examining the aqueous and alcoholic extracts of blood and urine, some non-crystalline matters have been found which sometimes manifested an acid reaction, and upon inci- neration left a carbonated alkali as a residue, thus presenting a remote similarity in deportment to the alkaline lactates.

From what substance could lactic acid be formed in the body of carnivorous animals? With the exception of fat, they partake of no non-nitrogenous matter in food, no such sub- stance, in fact, which is, so far as we know, capable of produ- cing lactic acid. Carnivorous animals partake of no sugar, no starch, no gum, no mucus; there is a total absence of the non-nitrogenous substances which form so large a part of the aliments of herbivorous and graminivorous animals.

The assumption, *à priori*, that neither the blood nor any other fluid in the body of carnivorous animals can possibly contain any lactic acid, has been positively established by the experiments of Enderlin*. Finally, Pelouze has proved that the experiments of Henry, who pretended he had detected lactate of urea in urine, are erroneous, and by no means to be relied upon.

Consequently, as our knowledge of this subject stands at present, the acid reaction of urine cannot proceed from lactic acid. And although processes of transposition take place in the healthy animal body, rendering insoluble substances solu- ble in the stomach and bowels, yet these are of a different kind from that process of putrefaction of caseine in milk which produces lactic acid from the elements of the non-nitrogenous elements.

Direct experiments prove that fresh urine, of a strongly acid reaction, and taken from various healthy individuals, when cautiously neutralized with water of barytes, does not retain in solution the least detectable trace of barytes. Now, as lactate of barytes is readily soluble in water, the urine would certainly, and of necessity, contain barytes, if its acid reaction were really owing to the presence of lactic acid. Upon the addition of the very first drop of water of barytes to urine a copious precipitate is formed; this precipitate contains urate and phosphate of barytes and of lime, and no detectable trace of barytes is found, even although only just so much water of barytes is added as to leave the urine still possessing a feebly acid reaction in the solution filtered from the precipitate.

Carbonate of magnesia and calcined magnesia act upon

* *Annalen der Chemie und Pharmacie,* vol. xlix. p. 317, and vol. l.

urine in precisely the same manner. If either of these sub-
stances be mixed with water, so as to form a milky fluid, and
be then added to urine with an acid reaction, the acid reac-
tion immediately disappears, and a very considerable white
precipitate is formed. The fluid now manifests a feebly alka-
line reaction, and contains a trace of magnesia in solution. It
is a remarkable circumstance that magnesia withdraws the
phosphoric acid from the urine so completely, that a mixture
of perchloride of iron and acetate of potass no longer indicates
a trace of phosphoric acid in the urine which has thus been
treated with magnesia.

Had lactic acid been the solvent of the phosphate of lime
and magnesia in the urine, one would have expected that a
corresponding amount of barytes, or of magnesia, would have
taken its place upon its separation. But, as I have already
observed, not a trace of barytes is found in solution when ba-
rytes has been employed for neutralizing the acid, and only a
slight trace of magnesia when the latter substance has been
used for the same purpose.

But, as urine contains a certain amount of alkaline phos-
phates, *i. e.* phosphate of soda and phosphate of potass, and
as barytes and magnesia form, with phosphoric acid, insoluble
salts, it might have been supposed that the neutral lactates
formed upon the neutralization of the urine with the two bases
had been decomposed, together with the phosphates of soda
and potass contained in the urine, and transposed themselves
anew, with these substances, into phosphate of barytes or of
magnesia, and into neutral lactate of potass or soda. In this
case neither barytes nor magnesia could remain in solution.
This circumstance, therefore, renders these experiments inde-
cisive, and leaves the question as to the presence or absence
of lactic acid in urine dependent upon more direct experi-
ments.

I employed putrid urine in my attempts to detect lactic acid,
because lactic acid is not destroyed by putrefaction, and it
must, therefore, of necessity, be present in putrid urine if it
really forms a constituent of fresh urine ; and because, if lactic
acid can at all be *formed* by the putrefaction of urine, from
matters containing previously no lactic acid, the question
whether lactic acid is to be reckoned among the constituents
of normal urine is at once practically decided ; or, more cor-
rectly speaking, the problem is proved to be impossible of so-
lution, since we possess no means of positively determining
which urine may be considered of a normal constitution, and,
on the contrary, which is to this extent abnormal. As mat-
ters at present stand, therefore, with regard to this subject, it

was immaterial whether the presence of lactic acid was detected in fresh or in putrid urine; if it was found to exist in the latter, this fact must be considered as a confirmation of Berzelius's examination of fresh urine; whilst its absence from putrid urine would justify us positively to assert that it does not form a constituent of fresh urine; and, moreover, that urine contains no substance giving origin, by putrefaction, to the formation of lactic acid.

I have come to the latter conclusion. I found it impossible to detect the presence of lactic acid in putrid urine; and if we examine somewhat more closely and minutely the experiments made by Berzelius, and from which he inferred the presence of lactic acid in urine, we find that not one of them amounts to a positive proof that lactic acid really forms a constituent of fresh urine.

The experiments which I made for the purpose of ascertaining the presence of lactic acid in putrid urine are the following :—

Putrid urine was first evaporated over an open fire, and afterwards to dryness in a water-bath; the residue was treated with a mixture of alcohol and sulphuric acid, which caused the solution of phosphoric acid, hydrochloric acid, and of lactic acid also, if this latter substance were really present. The fluid obtained was saturated with oxide of lead, and then filtered off from the phosphate, sulphate, and chloride of lead formed; the lead contained in the filtered solution was separated by sulphuretted hydrogen. The solution thus freed from lead, and which ought to have contained the lactic acid, had there been any present, was evaporated in a water-bath, and the residue treated with alcohol,—a quantity of common salt remained. In order to remove the soda from the alcoholic solution, effloresced oxalic acid was dissolved in the latter, at a high temperature, and the oxalate of soda formed was separated from the fluid by filtration; the fluid was then saturated with oxide of lead, which again gave rise to the formation and separation of chloride of lead. The solution was again freed from the lead which had dissolved sulphuretted hydrogen, then concentrated in the water-bath, and basic acetate of lead added in excess; a copious white precipitate was formed, from which the fluid was filtered off. This fluid must contain the lactic acid if any had been present in the urine; the lead which this fluid held in solution was precipitated by sulphuretted hydrogen, the fluid filtered off from the precipitate, concentrated in the water-bath and boiled with hydrate of barytes,—a quantity of ammonia was expelled by this

operation. After the decomposition of the ammoniacal salt the new-formed salt of barytes was cautiously decomposed by sulphate of zinc, and every possible means was applied to obtain from this fluid crystals of lactate of zinc, but without success—no trace could be discovered.

The white precipitate obtained by means of the basic acetate of lead contained hydrochloric acid, and a brown resinous substance, which, upon combustion, behaved like an animal substance.

In other experiments the putrid urine was boiled until all the carbonate of ammonia it contained was completely expelled; then, with addition of hydrate of lime to destroy the remaining salts of ammonia, evaporated to dryness, and the residue treated with cold water, which must have dissolved lactate of lime had any lactic acid been present in the urine. The aqueous extract was evaporated to dryness, and the residue again treated with alcohol; the fluid obtained contained a copious amount of lime combined with an organic acid; the lime was then removed by the addition of oxalic acid, and the excess of oxalic acid by the addition of oxide of lead; the minute trace of oxide of lead which dissolved was removed by means of charred blood. The fluid obtained was very acid; it contained hydrochloric acid, which was removed by the addition of oxide of silver; a portion of the fluid filtered off from the hydrochlorate of silver formed was saturated with oxide of zinc, and left to crystallize, but no lactate of zinc was obtained; the fluid settled into a dark-coloured resinous mass. Another portion of this acid fluid was evaporated in the water-bath; a quantity of acetic acid was expelled during the evaporation, and there remained at last only a very minute amount of a resinous matter, which upon calcination emitted a very fetid odour.

All the other experiments which I made in order to detect lactic acid in putrid urine, and a detailed description of which would be as tedious as useless, gave the same negative result. These experiments were usually made upon quantities of from forty to fifty pounds of urine, so that even a very minute amount of lactic acid, if really present in the urine, could not have escaped detection. All these experiments indicated the presence of an organic acid, but after the removal of all the inorganic acids and bases contained in the urine, this acid turned out to be a mixture of acetic acid with a brown resinous substance rich in nitrogen.

To remove any doubt on this point I took putrid urine, freed it from its carbonate of ammonia by evaporation, and

submitted it to distillation with dilute sulphuric acid, hydro-chloric acid, and oxalic acid; I obtained so much acetic acid as to be able to prepare with it many ounces of acetate of lead; from the salt of lead thus formed I was able to produce acetic æther and concentrated acetic acid, which, by mere agitation with æther, lost all urinous odour. Over and above all these tests, the salt of silver formed by this acid was subjected to analysis; it had crystallized like the common acetate of silver, in shining scales, of easy solution in hot water.

0·4319 grm. of this salt of silver yielded 0·2808 grm. of metallic silver.

0·9375 grm. yielded 0·4737 of carbonic acid, and 0·1508 of water.

According to this analysis it consists of,—

	Found.	Calculated.
Carbon	13·89	14·49
Hydrogen	1·78	1·78
Oxygen	19·32	19·24
Silver	65·01	64·49

and this agrees perfectly with the known composition of ace-tate of silver.

With respect to the presence of acetic acid in putrid urine, this was positively proved by Proust, in a paper published about twenty-four years ago*, and has subsequently been con-firmed by Thenard. This paper of Proust, as well as all the facts and experiments mentioned in it, seems to have escaped altogether the attention of those chemists who have been sub-sequently engaged in investigating the composition of urine. What I have to add to and correct in these works is, in part, contained in Proust's paper.

Thus, for instance, Proust observed that in the distillation of urine with sulphuric acid or hydrochloric acid, a certain amount of *benzoic acid* passes over together with acetic acid, and is deposited in crystals in the neck of the retort. I have found that, upon saturating with oxide of lead the acetic acid obtained in this process, a considerable amount of a white pre-cipitate is formed, consisting of pure benzoate of lead. More-over, if the concentrated putrid urine is mixed with some sul-phuric acid, and allowed to stand at rest for the space of se-veral days, a quantity of benzoic acid is obtained in shining brown scales; none of the various urines submitted to these experiments were free from this acid.

0·3544 grm. of the benzoic acid produced from urine yielded upon combustion 0·8805 of carbonic acid and 0·1618 of water. This gives in 100 parts—

* *Annales de Chimie et de Physique,* vol. xiv. p. 260.

			Found.	Calculated.
Carbon	.	.	68·58	69·10
Hydrogen	.		5·07	4·87
Oxygen	.	.	26·35	26·03

These proportions correspond exactly with the formula of benzoic acid.

With respect to the question of the presence of acetic acid and benzoic acid in fresh urine, no doubt can be entertained that the latter is not contained, as benzoic acid, in fresh urine, since it has been proved by the experiments of Ure and Keller, that crystallized benzoic acid becomes in the organism converted into hippuric acid, and appears in the urine as hippurate of soda. Now, as we also know that the hippuric acid contained in the urine of herbivorous animals passes into decomposition upon the putrefaction of their urine, and that benzoic acid is one of the products of this decomposition, we may safely and positively assume that the benzoic acid in putrid human urine has the same origin, and that consequently it must exist in fresh urine in the form of hippuric acid. This has been fully confirmed by the results of minute examinations. All the urine taken in this country from individuals living upon a mixed animal and vegetable diet, contains hippuric acid, besides uric acid, and about the same proportion of both acids. Hippuric acid may be obtained in the following manner, even from proportionally small amounts of fresh urine:—Fresh urine is evaporated in a water-bath to the consistence of a syrup; it is then mixed with some hydrochloric acid, and agitated with its own volume of æther, which latter substance dissolves the hippuric acid. It usually happens that the mixture does not separate spontaneously, but that the æther remains inclosed by the fluid, like froth; the separation of the æther takes place immediately upon adding to the mixture, after having allowed it to stand for an hour, one-twentieth part of its volume of alcohol. In this case the froth disappears, and the fluid separates into two layers; the upper layer contains the hippuric acid in solution, but it also contains urea, owing to the addition of the alcohol. This upper layer is carefully removed by means of a pipette or siphon, and agitated with small portions of water; the water removes the alcohol and the urea, whilst the hippuric acid remains in solution in the æther. By evaporating the æthereal solution the hippuric acid is obtained in crystals, which are usually of a yellowish or brown colour, imparted to them by the presence of a resinous substance, which may be easily and completely removed by means of charred blood.

In its pure state the hippuric acid obtained from human

urine presents the same long, shining, transparent, four-sided, obliquely-truncated prisms, by which the hippuric acid procured from the urine of animals is so easily detected and distinguished from benzoic acid. The hippuric acid of human urine is not volatile at the subliming temperature of benzoic acid; at a higher temperature it undergoes fusion, forming a brown-red liquid, and yields upon dry distillation the same products which common hippuric acid forms under the same circumstances, viz. a red-coloured oil, smelling like tonka-beans, ammonia, benzoic acid, and a copious residue of carbon. It dissolves in nitric acid at a high temperature, and yields, upon cooling, crystals of benzoic acid, owing to the decomposition which it undergoes.

From 0·499 grm. of hippuric acid produced from urine, 1·0791 of carbonic acid and 0·2317 of water were obtained. This gives for 100 parts—

	Found.	Calculated.
Carbon	59·47	60·89
Hydrogen	5·15	4·45

This analysis corresponds sufficiently with the calculated results to remove all doubt as to the nature of this acid; it will be perceived that it contains 10 per cent. less carbon than benzoic acid.

As far as our investigations into the composition of the aliments of man will allow us to judge, they contain no benzoic acid from which hippuric acid might have been formed, and as the urine of cows is invariably rich in hippuric acid, no matter whether the cows have been fed upon hay or upon beet-roots,—of which latter plants we know positively, from the results of several examinations, and from experience derived from the manufacture of beet-root sugar, that they contain no benzoic acid,—we can come to no other conclusion concerning the presence of hippuric acid in the urine of the herbivorous animals, and in the urine of persons living upon a mixed vegetable and animal diet, than that it is a product formed in the organism, to the formation of which the elements of their non-nitrogenous aliments give birth.

The presence of *acetic acid* in putrid urine does not warrant us to infer that this acid is also present in fresh urine; on the contrary, the experiments made with regard to this matter prove that fresh urine contains no acetic acid. I have treated fresh urine exactly in the same manner as putrid urine, and have by distillation with oxalic acid, for instance, obtained a fluid of a strongly resinous odour, but not possessing any

acid reaction. When employing sulphuric acid and hydro-
chloric acid the distillate was acid, but the acid reaction pro-
ceeded from hydrochloric acid.

I have, in the preceding experiments, several times alluded
to a nitrogenous resin-like substance, which is present in all
the fluids obtained in the examination of putrid urine. This
substance is a product of the putrefaction of the urine, and is
of an acid nature. If the residue remaining upon the evapo-
ration of putrid urine is treated with alcohol, and the result-
ing solution boiled with addition of hydrate of lime, until am-
monia ceases to be given off, then evaporated to dryness, and
the residue treated with cold alcohol, there remains a mass of
a light brown colour, which, at a high temperature, may be
kneaded like resin, and drawn out into long threads, and
which, upon combustion, emits a strong smell of urine, and
leaves a quantity of carbonate of lime as ashes.

If putrid urine is evaporated and sulphuric acid added, after
the lapse of some time, an oily, black-coloured substance se-
parates, which, being washed with water, assumes the appear-
ance of pitch. It is soluble in alcohol and in alkalies. Upon
distilling putrid urine with mineral acids this substance is ob-
tained in the residue to a considerable amount, although al-
tered in many of its properties. A portion of it, produced in
this manner, has,—apparently from the action of the acids at
a high temperature,—completely lost its solubility in alcohol,
whilst its solubility in alkalies remains unimpaired. From
this alkaline solution it may again be precipitated in black
flakes by acids. Proust has described these substances mi-
nutely, but as they cannot be procured from fresh urine, they
are, as products of the putrefaction of urine, of no interest in
the present investigation.

Just as benzoic acid and a nitrogenous substance are pro-
ducts of the putrefaction of hippuric acid in urine, I consider
acetic acid and the above-mentioned nitrogenous resinous sub-
stance to stand in a definite relation to each other; that is, I
consider them to be products of the decomposition of a sub-
stance containing the elements of both; and this substance
seems to be no other than the colouring matter of the urine.
Not the slightest trace of alcohol has ever been detected in urine
excreted after the use of spirituous liquors. It is evident,
therefore, that the acetic acid present in putrid urine cannot
owe its origin to the use of spirituous liquors. If sugar, or
sugar of milk, be added to fresh urine, and the urine is then
left to putrefy, the sugar or milk-sugar are found unaltered,
even after the lapse of three months. It is evident, therefore,

that these two substances, under the circumstances, cannot give rise to the formation of lactic acid, nor to that of acetic acid. The putrefaction of urea seems to be completely prevented by the presence of sugar, or milk-sugar; at least, in several experiments where one or the other of these two substances were present in the putrefying urine, the urea of the urine had not, even after the lapse of three months, acquired the property of effervescing with acids. I must here remark that a considerable amount of ammonia is formed in the process of putrefaction, owing to the resulting decomposition of another nitrogenous organic substance. This ammonia causes the protoxide of copper, formed upon testing the urine for milk-sugar with potass and sulphate of copper, to remain in solution, and to precipitate only upon the addition of acetic acid.

It follows from the preceding experiments and observations, that the organic acids in the urine of man are *uric acid* and *hippuric acid*, and another nitrogenous matter (most probably the colouring matter of urine), which latter substance, upon the access of air, resolves itself into acetic acid, and a substance resembling resin. Gay Lussac has already observed that it is only upon the access of air that urine passes into a state of putrefaction with absorption of oxygen*.

The considerations which I shall next adduce will prove sufficient to remove all doubt as to the cause of the acid reaction manifested by the urine of man.

It must be assumed as an irrefutable fact, that the inorganic bases present in urine, such as potass, soda, lime, &c., have entered the organism through the medium of the aliments.

Now, in the first place, with respect to the nutriment of man, assuming it to consist of meat, that is, of animal substances and of aliments prepared from the flour of the cereals, or of the seeds of leguminous plants, such as peas, beans, and lentils, we know exactly the constitution of the ashes of flesh,

* Urine which, as is well known, speedily putrefies and passes from an acid to an alkaline state, keeps very long in well-closed vessels, when it has scarcely been allowed to come into contact with the air. It retains, in such case, its transparency, acidity, and odour; it deposits only uric acid, and not the phosphate of ammonia and magnesia. If urine is left in contact with a small amount of air it speedily absorbs the oxygen present, and decomposition then ceases; but if a sufficient amount of oxygen is present, a considerable amount of carbonate of ammonia is formed, and phosphate of lime and phosphate of magnesia and ammonia precipitate. It will be perceived that the decomposition of urine has no analogy in this respect with fermentation, since this latter process once set afloat continues without the co-operation of oxygen.—Gay Lussac, *Ann. de Chimie,* vol. lxxvi. p. 245.

as well as of the ashes of the cereals and the leguminous plants. There are no carbonated alkalies among the soluble salts of these ashes, but bibasic and tribasic phosphates of the alkalies, phosphate of soda or of potass, or both at the same time in varying properties. We might have supposed that the food in the stomach when in a state of solution would manifest the reaction of those salts which form the constituents of the ashes of the aliments partaken of. Now, in fact, all the bibasic and tribasic phosphated alkalies have an alkaline reaction, and yet nevertheless the reaction of the chyme is acid.

According to the best authenticated investigations, the acid reaction of the gastric juice is owing to the presence of free hydrochloric acid, and for the origin of this acid we must refer to common salt.

It is obvious that common salt undergoes decomposition in the organism, and becomes resolved into hydrochloric acid, which we find in a free state in the gastric juice, and into soda. This latter substance is, previous to the chyme having assumed the form adapted for its transformation into blood, that is, previous to its being changed into chyle, reconducted into the alimentary canal through the medium of the bile, the only compound of soda which we know with certainty to exist, as such, in the organism.

When we compare the composition of flesh, or of coagulated albumen, with that of the blood, we find a very considerable difference with regard to the inorganic constituents of these substances. The serum of the blood is miscible with water in all proportions, and has an alkaline reaction ; the muscular fibre is insoluble, and has no alkaline properties. The ashes of blood contain principally alkaline phosphates, whilst the ashes of muscular fibre contain a far larger proportion of phosphate of lime. It is obvious that upon the transformation of the blood into muscular fibre, the greater amount of alkaline phosphates re-enter the circulation, whilst a certain amount of phosphate of lime remains in the organs in a state of chemical combination. When we consider that the insolubility of flesh, and of the cellular tissue in their natural state, depends upon the presence of the insoluble phosphate of lime, and that by the removal of the phosphate of lime the power of dissolving in alkaline fluids is restored to or increased in the remaining constituents containing nitrogen and sulphur, the necessity of the presence of free hydrochloric acid in the formation of chyme becomes immediately manifest. No mineral acid, and still less no organic acid, such as lactic acid or acetic acid, possesses the power of dissolving boiled meat

and albumen, or any aliment similar to these, in the same degree as hydrochloric acid in the dilute state in which it exists in the gastric juice; and this dissolving power is, moreover, quite independent of the other substances present in the gastric juice, for those other substances do not act as solvents, they merely exercise an accelerating influence upon the process of solution. Hydrochloric acid in contact with calf's stomach, acts upon coagulated albumen simply with increased rapidity; the solution ensues with equal completeness, whether we use hydrochloric acid with calf's stomach, or pure hydrochloric acid diluted to the same extent. The difference between the processes in the two cases is simply a difference of time; the solution in the former case requires for its completion only the fifth part of the time necessary in the latter case; this fact has been confirmed by all the experiments on artificial digestion with access of air, and therefore of oxygen. The action of hydrochloric acid on bones will best illustrate the action which this acid exercises upon phosphate of lime. Microscopical investigations have shown us that the phosphate of lime is contained in bones in chemical combination with gelatine, and not deposited in gelatinous cells. When bones are digested in very dilute hydrochloric acid their phosphate of lime is dissolved out within a few per cents., and the residuary gelatine, which under other circumstances requires several hours for its solution in boiling water, now dissolves in the space of a few seconds.

Disregarding altogether the solvent power which hydrochloric acid exercises upon the animal substance itself, there is no doubt that it destroys the combination of the organic substance with phosphate of lime, and by that means increases its solubility.

When the hydrochloric acid (in the stomach) has exercised its solvent action upon the aliments, and the latter have passed into a state of solution, the soda, which originally entered the organism in combination with the hydrochloric acid, that is, as common salt, rejoins the hydrochloric acid during the preparation of the chyme, and previous to its transformation into chyle; the soda and the hydrochloric acid thus re-united combine again and form common salt. Chyle and lymph have no longer any acid reaction, but, on the contrary, they manifest alkaline properties.

The alkaline reaction of the lymph, chyle, and blood of man, and of the carnivorous animals, cannot be owing to the presence of a free alkali, as is evident from the preceding observations; for the nutriments of man, and of the carnivorous as well as the graminivorous animals, contain no free alkali,

nor any salt formed of an alkaline base and an acid which might be destroyed in the organism, by the vital process, and thus cause the alkaline base to be liberated. The blood must contain the same salts as exist in the aliments. With the exception of common salt, nothing is added during the digestion of the aliments. We have seen that this substance undergoes decomposition in the upper part of the digestive apparatus, being resolved into free soda and free hydrochloric acid; but we have also seen that the liberated soda rejoins the hydrochloric acid during the preparation of the chyme, and previous to the transformation of the latter into chyle; that is, when the acid has performed its function, namely, the *solution* of the aliments; the salt formed by this combination, that is, common salt, has neither an acid nor an alkaline reaction. The salts with alkaline reaction contained in meat, flour, or grain, are alkaline phosphates. It is obvious that the alkaline reaction of the chyle, lymph, and blood of animals feeding upon animal and vegetable substances, can only be derived from their alkaline phosphates. The serum of the blood can only be considered as a combination of albumine with an alkaline phosphate; the fibrine of the blood, or the fibrine of the muscular fibre, is a combination of albumine with phosphate of lime.

The bibasic phosphates of soda and of potash are, in many respects, highly remarkable salts; although of a tolerably strong alkaline reaction, yet they exercise no destructive action upon the skin or upon organic formations; they possess all the properties of the free alkalies without being such; thus, for instance, they absorb a large amount of carbonic acid, and this in such a manner that acids produce effervescence in a saturated solution of this kind, just as they would in alkaline carbonates; they dissolve coagulated curd of milk or cheese, as well as coagulated albumen, into clear fluids with the greatest facility, just as caustic or carbonated alkalies do. But of still greater importance in relation to the secretion of urine is their deportment towards hippuric acid and uric acid. Hippuric acid dissolves with the greatest facility in water to which common phosphate of soda has been added; uric acid possesses the same property at a high temperature; the phosphate of soda, in this process, loses its alkaline reaction completely upon the addition of uric acid and hippuric acid, and assumes an acid reaction. The acid nature of the urine of man, and of the carnivorous and graminivorous animals, is thus explained in a very simple manner.

There are but two principal channels through which the salts entering the organism with the aliments can effect their

exit from the body; viz. they must either be carried off in the fæces or in the urine. The most simple experiments show that soluble salts are carried off by the fæces only when the amount of salt contained in the fluids in the intestines is larger than that contained in the blood; if the amount of salt in these fluids is equal or inferior to that of the blood, the soluble salts are re-absorbed by the absorbing vessels of the intestinal canal, and enter the circulation, and are then removed from the body by the urinary organs and channels. If the amount of salt contained in the intestinal canal is larger than that contained in the blood, the salts exercise a purgative action.

If, after previous evacuation of the rectum, a weak solution of common salt (one part of salt to sixty parts of water) be taken by means of a clyster, no second evacuation will take place; the fluid is absorbed, and all the salt is found in the urine. This experiment yields the most convincing results if ferrocyanide of potassium is substituted for common salt; in this case, the first urine excreted after the injection of the saline solution, and frequently even after so short a time as fifteen minutes, contains so copious an amount of ferrocyanide of potassium, as to yield, upon the addition of persalts of iron, a copious precipitate of prussian blue.

The influence which salts in general exercise upon the secretion of urine is in the highest degree worthy of attention. It is a well-known fact that a very speedy emission of urine takes place, in healthy individuals, after drinking fresh pump-water. If ten glasses of water, of from six to eight ounces each, containing no more than 1·500th of its amount in salts, be drunk at short intervals, an emission of urine of the usual colour will, after the lapse of about ten minutes, follow the second glass, and from eight to nine evacuations of urine will generally occur in the course of an hour and a half. The urine, in this experiment, emitted in the last evacuation, will be clear and colourless, like pump-water, and the amount of salts it contains is little more than is contained in pump-water. There are individuals who are capable of thus imbibing from six to eight quarts of water consecutively without any inconvenience.

But the case is quite different with water possessing an amount of salts equal to that of the blood; if even as little as 1-100th part of common salt be added to pump-water, and from three to four glasses drunk, no evacuation of urine will take place, even two hours after drinking. It is almost impossible to drink more than three glasses of this saline water, for it weighs heavily on the stomach, as if the absorbent vessels had no power of taking it up. This obviously arises from

the fluid within the channels of circulation, *i. e.* the blood, and
the fluid without these vessels, *i. e.* the saline water, not exer-
cising any physical action upon one another, *i. e.* not inter-
mixing by endosmose or exosmose.

Water containing a larger amount of salts than the blood,
such as common sea-water, for instance, and even the weaker
kinds of saline mineral waters, exercise again a different action
from that of pump-water mixed with 1·100th of common salt;
not only no emission of urine takes place after the imbibition
of such saline water, but water exudes from the circulating
vessels into the intestinal canal, and, together with the saline
solution, is carried off through the rectum; purgation takes
place, attended with much thirst, if the saline solution be in
some measure concentrated.

Considering that a certain amount of salts is absolutely ne-
cessary to constitute normal blood, we may deduce from these
observations and experiments (which any one may easily imi-
tate and verify upon his own person) that the physical condi-
tion of the tissues or of the blood-vessels opposes an obstacle
to any increase or decrease of the amount of salts in the blood;
and thus that the blood cannot become richer or poorer in
salts beyond a certain limit.

Fluids containing a larger amount of salts than the blood
remain unabsorbed, and leave the organism through the rec-
tum; fluids containing a smaller amount of salts than the
blood enter into the circulation, absorb, and remove from the
organism, through the urinary channels, all the soluble salts
and other substances which do not belong to the constitution
of the blood; so that, finally, only those substances remain in
the organism which exist in chemical combination with the
constituents of the blood, and which, therefore, are incapable
of being secreted by the healthy kidneys.

I have convinced myself, by careful and minute examina-
tions, that urine emitted after drinking a copious amount of
water invariably contains a somewhat larger amount of salts
than the water which has been drunk; whilst the amount of
phosphates contained in the last emitted portions of the urine
is extremely minute, and no longer detectable by the ordinary
tests. It is therefore obvious that all the salts, without ex-
ception, contained in the urine, are to be considered as acci-
dental constituents of the blood, which are excreted and re-
moved from the organism precisely because they no longer
form part of the normal constitution of the blood. The phos-
phates emitted with the urine were, previously, constituents of
substances which have been decomposed in the vital processes,
or they existed as constituents of the blood, but upon its trans-

formation into living tissues they were not admitted into their composition, they were not required in the constitution of the latter.

Now, among the products of the vital processes, which, together with the soluble phosphates, are removed from the organism through the urinary organs and channels, there are two organic acids, namely, *uric* acid and *hippuric* acid, both possessing the property of combining with the soda or potash of the alkaline phosphates, and acquiring in the combination a higher degree of solubility than they possess, *per se*, at the common temperature of the body. It is obvious that by the accession of these two acids, and by their action upon the phosphates of soda, an urate and a hippurate of soda must be formed on the one hand, and an acid phosphate of soda on the other; and that, consequently, the urine must acquire an acid reaction.

But the presence of these two acids in the urine is not the only cause of its acid nature; there exists another cause which tends powerfully to maintain and increase it.

According to the preceding remarks we ought to find in the urine all the soluble salts of the food, as well as a small amount of the phosphate of lime, which is soluble to a certain extent in acid fluids, together with magnesia. The amount of these latter substances will be in proportion to their solubility in acid phosphate of soda. The other insoluble salts of the aliments we ought to find in the fæces. In other words, assuming that the materials composing the aliments become converted into oxygen compounds, that is, are *burnt* in the organism, we ought to find in the urine all the soluble salts of their ashes, and in the fæces all the insoluble salts. Now, upon comparing the constitution of the ashes of the blood or of the aliments, or, rather, the salts contained therein, with those contained in the urine, we find that there exists a striking difference between their respective amount of *sulphates*.

According to the analyses of the ashes of the grains of wheat and rye *, the urine of an individual feeding exclusively upon bread ought not to contain a single trace of a sulphate, whilst the urine of an animal fed upon peas or beans ought to contain sulphates together with phosphates in the proportion of 9 of the former to 60 of the latter. Finally, as flesh contains no soluble alkaline sulphate (broth does not yield any precipitate of sulphate of barytes when tested with salts of barytes), the urine of carnivorous animals ought to be equally free from soluble sulphates. We find, on the con-

* *Annalen der Chemie*, vol. xlvi., p. 79.

trary, that the urine of man, according to the most correct analyses, contains afar larger proportion of sulphates than the aliments partaken of; nay, even that the amount of the sulphuric acid received into the system must, in many cases, be equal or superior to that of the phosphoric acid contained in the aliments. According to the analysis of human urine made by Berzelius and Lehmann, the amount of the sulphates present in urine is nearly double that of all the soluble phosphates together. Hieronymi found the amount of sulphate of potash contained in the urine of the tiger, the lion, and the leopard, compared with that of the phosphates, to be as 1 to $7\frac{1}{2}$. It can be distinctly and positively proved that these salts have not been partaken of in such proportions. But we now know the origin of the greatest portion of the sulphuric acid contained in the urine; this acid has entered the organism with the food, not in the form of a sulphate, but as *sulphur*.

Gluten*, vegetable casein, flesh, albumen, fibrine, and the cartilages and bones, contain sulphur in a form quite different from the oxygen compounds of this substance. This sulphur is separated as sulphuretted hydrogen during the putrefaction of these substances; it combines with the alkalies, operating upon these animal substances, and may be obtained from such combinations in the form of sulphuretted hydrogen by means of stronger acids.

Now we know, from the experiments of Wöhler, that the soluble sulphurets become oxidized in the organism; and that thus, for instance, sulphuret of potassium becomes converted into sulphate of potash; and it is therefore unquestionable that the sulphur of the constituents of the blood, derived from the aliments, or, what comes to the same point, the sulphur of the transformed tissues, becomes finally converted into sulphuric acid by the oxygen absorbed in the process of respiration, and thus that in the urine it must appear in the form of sulphates; and from this cause the original amount of these salts contained in the aliments becomes increased. The alkaline base which we find in the urine, in combination with this sulphuric acid, is supplied by the soluble alkaline phosphates; and the latter, in consequence of the loss of part of this base, are converted into acid salts.

By these considerations and views respecting the cause of the acid reaction of urine, I have been induced to prepare

---

* Dietrich has examined gluten (in the laboratory of Giessen) with regard to its amount of sulphur; he found wheat-gluten to contain from 0·033 per cent. to 0·035 per cent. of sulphur, exactly the same proportion as is contained in albumen or fibrine.

an artificial urine, which possesses the properties of natural urine, even although sulphuric acid be altogether excluded.

If 40 grains of dry phosphate of soda (or 90 grains of the crystallized salt, $PO_5 \, 2 \begin{cases} NaO \\ H \, O \end{cases} + 24 \, Aq.$) be dissolved in one pound of water, a fluid will be obtained having an alkaline reaction; if to this fluid we add 15 grains of uric acid, and 15 grains of hippuric acid, and the mixture is heated, both acids will completely dissolve, imparting a strong acid reaction to the fluid. The solution thus prepared does not deposit a trace of uric acid at a temperature from $37° - 38°$ ($= 98°$, $100°$ Fahrenheit $=$ the heat of the blood); nay, it is even only several hours after complete refrigeration that a sediment is formed, consisting of uric acid containing soda; this sediment is of an analogous form to that deposited by natural urine after standing at rest for a long time. Upon collecting this sediment, in one of my experiments, after the lapse of twenty-four hours, I found that it weighed $7\frac{1}{2}$ grains, so that there remained still in solution $22\frac{1}{2}$ grains of the organic acids. Dilute mineral acids produce immediately, in the fluids filtered off from the sediment, a precipitate of uric acid.

Proust, Prout, and all the other chemists who examined the urine previous to, or about the same period as Berzelius, ascribed its acid reaction to the uric acid or phosphoric acid; hippuric acid was not known as a constant attendant upon uric acid.

Proust says, at page 260 of the paper above referred to, " it is the phosphoric acid which imparts to the urine its acid properties. If urine is evaporated to dryness, then treated with alcohol, urea, the colouring resinous principle, and phosphoric acid dissolve out; the two former substances exercise no action upon lime-water, but the phosphoric acid causes a white precipitate in lime-water, as well as in solutions of salts of lead; this precipitate is formed upon adding to lime-water, or to solutions of salt of lead, a few drops of the alcoholic solution."

With regard to lactic acid, Berzelius remarks, at page 421 of his Manual, " Lactic acid is a general product of the spontaneous destruction of organic matter within the organism, it is therefore contained in all the animal fluids. It is formed in the largest proportion in the muscles, becomes saturated by the alkali of the blood, and, in the kidneys, is again separated from the alkali. It is principally this acid which forms the free acid of the urine; and although this contains acid phosphate of ammonia and acid phosphate of lime, yet these

substances owe their origin and formation to the circumstance of the lactic acid sharing the bases with the phosphoric acid.

" In the course of an investigation into the constitution of urine, in the year 1807, I found lactic acid as a constituent of urine; this acid had not, up to that period, been counted among the constituents of urine; and several chemists having, on very slight grounds, asserted it to be acetic acid, I submitted it to the test of a new examination; the results of this examination led me to conclude that lactic acid could not be confounded with acetic acid, and that the acid found was lactic acid, and not acetic acid.

" The lactic acid contained in the urine is the principal solvent for the phosphate of lime; this may be easily seen from the fact, that after having extracted from exsiccated urine the free lactic acid by means of alcohol, the residue indeed contains acid phosphates, but upon redissolving it in water it leaves the greatest portion of the phosphate of lime as an insoluble residue."

I deem it scarcely necessary to examine the reasons which induced Berzelius, thirty-seven years ago, to assume the presence of lactic acid (a substance the properties of which were then totally unknown) in urine and in animal fluids. The then prevailing idea that lactic acid must be a product of the decomposition of all animal fluids, because it is formed in milk, has now turned out to be totally unfounded. Milk contains neither lactic acid nor lactates, the formation of lactic acid in milk being dependent upon milk-sugar.

It was evidently from entertaining these incorrect notions that Berzelius was led to assume the presence of lactic acid in certain non-volatile fluids of the urine, which have an acid reaction, and contain organic substances beside phosphoric acid; there is no doubt that the presence of lactic acid in these fluids was highly probable, if lactic acid were truly a constant product of the putrefaction of *all* animal fluids. But when we consider that the properties of lactic acid were then so little known that many chemists even disputed the fact of its existence, and asserted it to be nothing more than acetic acid in disguise, we see clearly that there could exist no analytical grounds, at that time, whereon to found a positive assertion of its presence in urine and other animal fluids. At present there is far less reason for such a supposition, since now we have closely examined and studied this acid, and have obtained a correct knowledge of its properties, and yet we are incapable of detecting even a trace of it in any animal fluid.

In the nineteenth volume of Poggendorff's *Annalen der*

Physik und Chemie, p. 26, Berzelius says, " Leopold Gmelin, whose lucid researches occupy so distinguished a place in animal chemistry, has almost put himself at the head of those who positively deem lactic acid to be acetic acid."

Proust and other chemists had asserted the organic acid contained in urine to be acetic acid, and had thereby induced Berzelius to examine more minutely the properties of lactic acid, in order to determine its difference from acetic acid. Thus, twenty-three years after the supposed detection of lactic acid in urine, the very existence of this acid remained still a matter of doubt.

Now, although Berzelius states (p. 422 of his work) that the assertion of the presence of acetic acid in urine, on the part of other chemists, induced him to enter into a comparative examination of lactic acid and of acetic acid, this examination was not made, as one would suppose it would have been, with the organic acid of the urine, asserted by him to be lactic acid, but, on the contrary, with real lactic acid produced from milk; this investigation proved the existence of lactic acid in milk, but it did not follow from this that lactic acid is present in urine, since, as I just now remarked, milk had been used for these experiments and not urine. [No lactic acid can be produced from broth by the method recommended by Berzelius.]

Finally, concerning Berzelius's view, that lactic acid is the solvent of the phosphate of lime in the urine, and that this substance becomes insoluble whenever the free lactic acid is removed by alcohol, we need merely recollect the well-known property of acid phosphate of lime to be resolved by alcohol into phosphoric acid, which dissolves in the alcohol, and into the common insoluble bone-earth, to perceive at once the fallacy of this inference. We know that it is precisely upon this property that the separation of phosphoric acid from lime depends. Alcohol dissolves phosphoric acid and hippuric acid out of urine evaporated to dryness, but no lactic acid.

After the preceding remarks, it will not be difficult to appreciate the true value of the evidence upon which the assumption of the presence of lactic acid in urine and in animal fluids depends; if I add that the method followed by Berzelius in his experiments (decomposition of urea by boiling with milk of lime, &c.) can no longer be considered perfectly exact and correct, it will be admitted that, so far as our present experience and observations enable us to judge, no kind of putrefaction, such, for instance, as the formation of lactic acid, can take place in the organs of the living and healthy animal body.

When,—notwithstanding the esteem I have always enter-

tained, and shall always entertain, for my highly-honoured friend whose views I am controverting,—I thus enter into a searching and critical investigation respecting lactic acid, both historically and analytically, it is for the interest of science, and to ascertain the truth upon this highly important subject. The notion of the presence of lactic acid in the animal body, in the gastric juice, in the milk, in the blood, in the urine, is so extensively diffused, and the part which has been assigned to this acid in digestion, in the respiratory process, &c., is so great and comprehensive, that the proof of its non-presence and, consequently, of the impossibility of assigning to it any share whatever in the mysterious processes of the animal organism, was well deserving of being supported by every admissible reason.

The diffusion of lactic acid, or of that acid which was assumed to be the lactic, in the animal body, depends, moreover, only upon the delusion existing, from the beginning, with regard to this acid.   Berzelius states (p. 460 of his work), when alluding to Hieronymi's analysis of the urine of the lion, in which that chemist asserted that he had found acetic acid, " I have taken the liberty here to substitute the term lactic acid for acetic acid, for reasons which will be apparent from the preceding remarks."   Now, the reason for this substitution was, that Berzelius, in an experiment made for this especial purpose, had found that fresh urine yields no acetic acid upon distillation with a mineral acid,—a fact which, as already stated, I have had occasion to confirm by my own experiments.   This substitution of lactic acid for acetic acid, where the presence of the latter had been asserted previously, was, in the subsequent editions of his Manual, consistently carried through in all the statements of analyses of animal matter. The circumstance that in milk, as well as in sour-krout, upon acidification, acetic acid is invariably formed, besides lactic acid, renders the propriety of this substitution the more doubtful, since the acetic acid was thus of course wholly disregarded.

It follows, from all we have hitherto stated, that the acid nature of the urine of carnivorous animals, as well as of that of man, depends upon the nature of the *bases* partaken of in the aliments, and upon the particular form of their combinations.   In the flesh, blood, and other parts of animals, as well as in the grains of the cereal and leguminous plants, there exists no free alkali.   The alkali which these substances contain is invariably combined with phosphoric acid : the acids formed in the organism by the vital process, namely, sulphuric acid, hippuric acid, and uric acid, share the alkali amongst them,

and this of course must give rise to the liberation of a certain amount of phosphoric acid, or what comes to the same point, to the formation of a certain amount of acid phosphates of soda, lime, and magnesia. The proportional amount of the liberated phosphoric acid varies with the temperature ; at a higher temperature the phosphate of soda dissolves a larger amount of uric acid and hippuric acid than at a lower tempe-rature,—at from 37°–38° more than at 15°. It is owing to this that urine, upon refrigeration, sometimes deposits uric acid, or urate of soda in a crystalline state, which of course can only take place by the uric acid, at a lower temperature, restoring to the phosphoric acid the soda or potash which at a higher temperature it had withdrawn from it. At the com-mon temperature phosphoric acid decomposes urate of soda, whilst at a higher temperature uric acid decomposes phos-phate of soda. When urine, containing uric acid and mani-festing an acid reaction, forms no sediment upon cooling, it shows that the amount of the phosphoric acid and that of the uric acid exactly balance each other with regard to their affi-nity for soda. Had there been present a larger proportion of uric acid, this would have separated upon cooling ; whilst, on the other hand, the presence of a preponderating propor-tion of phosphoric acid would likewise have caused the pre-cipitation of uric acid, because the affinity of the former for soda would then exceed that of the latter. This explains the circumstance that urine in certain states, when, from some cause or other, its amount of sulphuric, hippuric, or other acid becomes increased, precipitates a larger proportion of uric acid than urine in its normal state. The solubility of uric acid in urine must decrease in proportion as the amount of the other acids present in the urine increases, be-cause those acids share the soda with the uric acid ; and of course the larger the amount of soda which combines with these other acids the less comes to the share of the uric acid. It is likewise owing to this that uric acid very frequently pre-cipitates from urine upon the addition of mineral or other acids, and that urine of a turbid, whey-like appearance, from the presence of uric acid, frequently manifests a far more strongly acid reaction than normal urine.

Now, bearing in mind that the use of alkaline citrate (Gil-bert Blane), of neutral paratartrate of potash, bi-tartrate of potash, tartarized soda, acetates of potash and soda, and tar-tarized borax, renders the urine alkaline by creating in it an amount of carbonated alkali ; and that, likewise, after the eating of fruit, such as cherries, strawberries, &c., the urine is of an alkaline nature, inasmuch as these fruits contain alka-

lies combined with vegetable acids, it is obvious that the acid reaction of healthy urine is purely accidental, and that urine of an alkaline or neutral reaction cannot be considered as a symptom of a diseased condition of the body. All vegetable aliments, without exception, tubers, roots and leaves, potatoes, turnips, greens, &c., contain alkalies in combination with vegetable acids; potatoes, for instance, contain alkaline citrates; turnips, alkaline racemates and oxalates, &c. All these plants yield, upon incineration, more or less strongly alkaline ashes, the bases of which were contained in the living plants as salts of vegetable acids.

It is obvious that by adding these vegetables to a meat diet, to bread and to other aliments prepared from flour, the nature of the urine must become thoroughly altered; for the alkalies which these vegetables contain in combination with vegetable acids, enter the urine, in the form of carbonated alkalies, and neutralize the acids, of whatever kind, which may be present. When partaken of in a certain proportion they render the urine neutral; when partaken of in a larger proportion they impart to it an alkaline reaction.

The urine of all animals feeding upon vegetables, such as grass, herbs, roots, &c., has an alkaline reaction. The urine of the horse, of the cow, of the sheep, of the camel, of the rabbit, of the guinea-pig, of the ass, &c., is alkaline; it contains alkaline carbonates, and acids produce in it a lively effervescence.

The acid, neutral, or alkaline reaction of urine of healthy individuals does not depend upon any difference in the processes of digestion, respiration, or secretion, in the various classes of animals, but upon the constitution of the aliments, and upon the alkaline bases which enter the organism through the medium of these aliments. If the amount of these bases is sufficiently large to neutralize the acids formed in the organism, or supplied by the aliments, the urine is neutral; whilst it manifests an alkaline reaction when the amount of alkaline bases thus supplied to the organism is more than sufficient to neutralize the acids; but in all these cases the urine accords with the nature of the aliments taken.

The inorganic bases and acids contained in the urine were, with the exception of sulphuric acid, which joins them in the organism, constituents of the aliments. The amount of inorganic bases and acids emitted through the urine in twenty-four hours must, in adult individuals, be equal to that of these bases and acids supplied to the organism, during the same period, through the medium of the aliments.

From these data, it follows necessarily,—first, that the ana-

lysis of urine, when made without respect to the inorganic salts, acids and bases supplied by the aliments, teaches nothing whatever, and by no means justifies us in drawing therefrom any physiological or pathological inference ; secondly, that from the nature of the ashes of the aliments we are able to determine, positively, the constituents of the urine emitted ; and thirdly, that only when these latter have been distinctly ascertained can we expect to derive, from the analysis of the urine, any correct information with respect to the inorganic matters which have come to be present in it through processes of disease ; this, at least, is the *chemical method* of quantitative investigation.

Bearing in mind that the urine contains the soluble constituents of the ashes of the aliments, whilst the fæces contain the insoluble part of these constituents, we may from an accurate knowledge of both, at once determine in which urine soluble alkaline phosphates must be present, and in which they cannot exist. The ashes of all seeds, and of flesh and blood, contain a certain amount of soluble and insoluble phosphates, whilst the ashes of vegetables contain no free alkaline phosphate, but only insoluble phosphates. These vegetable ashes contain far more lime and magnesia than is required for the neutralization of the phosphoric acid present. Hence, upon incinerating a plant, together with its seed, and lixiviating the ashes, we find in the fluid obtained no alkaline phosphate, although the lixivium of the ashes of the seeds, when incinerated and lixiviated by themselves, yields a considerable amount of these phosphates ; the excess of lime and magnesia contained in the leaves and the straw enter here into combination with the phosphoric acid of the soluble alkaline phosphates, forming an insoluble compound.

It will now be understood why the alkaline phosphates are generally absent from the urine of herbivorous animals, and also why, in certain cases, they may be found in the urine of these animals. If the nutriment of these animals contains no soluble phosphates, their urine cannot contain any, whilst if we add a certain proportion of grain to their food, the alkaline phosphates may be detected in their urine. Thus it is obvious, likewise, that the soluble phosphates in the urine of man are merely accidental constituents, and that by simply adding lime or magnesia to the aliments, and thus assimilating the constitution of these aliments to that of the food of herbivorous animals, the urine must become altered in its nature and properties. The knowledge of the influence which alkalies, magnesia and lime, or acids, exercise upon the properties of the urine, or, in other words, upon the secretory pro-

cess of the kidneys, in the healthy organism, is of the highest importance for the curing of diseases.

I believe that there is now required only a small number of good and correct observations to establish a fixed rule for the remedies necessary in various cases. Future properly-directed experiments will prove whether sanguification is absolutely dependent upon the presence of alkaline phosphates or not; we shall be able to determine whether weak solutions of alkaline phosphates are not the best solvents for uric acid deposited in the bladder; and likewise what is the influence which aliments rich in sulphur, such as mustard, for instance, exercise upon the separation of uric acid in the bladder, in consequence of the formation of sulphuric acid. At any rate, we may, by a judiciously selected diet, alter with positive certainty, and at pleasure, the nature of the urine; we may, without causing any injury to health, keep it alkaline for a long time by adopting a vegetable diet; and this is certainly the first condition necessary to ensure the entire prevention of the formation of uric acid, as is the case with the herbivorous animals. By its combination with an alkaline base, uric acid must in the organism resolve itself into its ultimate oxygen compounds with the same facility as other organic acids, if the physician prohibits all substances to be taken as food which, like wine or fat, take possession of the oxygen necessary for the transformation of uric acid into carbonic acid and urea.

The carbonated alkali in the urine of herbivorous animals is separated from the blood by the kidneys; the urine derives it from the blood; it is certain, therefore, if we examine the blood one hour or a few hours after the animal has partaken of food, we must find in it this alkali in the same state as it is found in the urine, and that at other periods of the day the ashes of the blood may not contain the least trace of free alkali. But the free alkali does not cooperate in the vital process in the animal organism; or, if it *is* necessary in this process, the part which it has to perform may be undertaken with the very same effect by the bibasic and tribasic alkaline phosphates.

In like manner, when we are contemplating the presence of hydrochloric acid in the gastric juice, we must remember that the alkaline bases, soda, potash, lime, magnesia, are present in the aliments whilst in their natural state, invariably in the form of salts; that is, in combination with phosphoric acid or with organic acids. When, therefore, in the digestive process, hydrochloric acid is supplied by the gastric juice, the first action of this acid is confined to the decomposition of these salts;

the hydrochloric acid withdraws lime from the phosphate of lime; potash, or soda, chloride of calcium, or chloride of sodium or potassium, is formed on the one hand, and acid phosphate of soda or potash, or acid phosphate of lime, on the other; or acetic acid, paratartaric acid, or citric acid, are liberated by the decomposition of the salts of these vegetable acids contained in the aliments.   At a certain stage of digestion the chyme will, according to the nature of the food partaken of, contain acid phosphates or free vegetable acids; and it is only upon the supply of gastric juice continuing, that thus, upon the amount of the hydrochloric acid increasing, we may detect, by analysis, free hydrochloric acid in the chyme; the gastric juice taken from an empty stomach contains invariably free hydrochloric acid or acid phosphates.

With respect to the essential organic constituents of urine, I have already treated of the origin of urea and uric acid in my " Chemistry applied to Physiology and Pathology;" and I will here merely add a few words on the presence of ammonia in urine.

Ammonia is a product of the putrefaction of nitrogenous substances, and as such might be a purely accidental constituent of the healthy animal body, and of its secretions.   In consequence of certain processes which take place in the organism independently of the vital process, all the fluids in the body may of course become rich in ammonia, or in ammoniacal salts.   Healthy urine contains only very minute or doubtful traces of ready-formed ammonia, and these traces existed probably already in the food partaken of.   Fresh urine evolves ammonia when treated with alkalies, but it yields no precipitate with chloride of platinum.   Dr. Schlossberger made certain experiments to this effect in the laboratory at Giessen; upon treating fresh urine with chloride of platinum, and allowing the mixture to stand at rest during the night, crystals were formed in the urine, which, upon examination, manifested all the properties of chloride of platinum and potassium.   The amount of ammonia formed in the healthy organism is likewise very minute, not being sufficient even to neutralize the acid from which proceeds the acid reaction of urine and of saliva. We cannot assume the presence of any ammoniacal salt in the urine of herbivorous animals, which contains fixed or alkaline carbonates.

The carbonate of ammonia which Chevreul found as a constituent of the urine of the camel, was either a product of putrefaction, or an accidental constituent of this urine, since the carbonate of ammonia is a volatile substance, and must find

the skin and lungs a more direct way for its exit from the organism than the kidneys.

Experiments for the determination of the amount of ammonia in the urine of healthy individuals may become of importance in judging of pathological states; for in fevers, and other diseases, the amount of ammonia in the urine increases considerably. It is possible that, by analysing the urine, we may, in the increasing or decreasing amount of ammonia, obtain a measure for the alterations which take place in diseases. But the salts of potash, which are rarely absent, as well as the ammonia which is formed by the action of chloride of platinum upon the organic constituents of urine, render this reagent (the chloride of platinum) very unsafe for determining the increasing or decreasing amount of ammonia in the urine during disease. The magnesia salts would perhaps answer this purpose better; the quantitative examinations made with salts of magnesia are inferior to those made with chloride of platinum, but they are exact enough for the purpose of comparison.

LXXVII. *Intelligence and Miscellaneous Articles.*

ON THE ESSENTIAL OIL OF BETULA LENTA AND ON GUALTHERINE. BY W. PROCTER.

THE author has found that the essential oil, which is obtained by the distillation with water of the *Betula lenta* (sweet birch or mountain mahogany), which is so common in North America, and which gives to some of its flowers their agreeable odour, which resembles that of gualtheria oil, is *identical* with the latter; but that the bark evolves this odour only when in contact with water, whilst originally it contains an inodorous body only, viz. gualtherine, which is converted into the essential oil when water is present, by the reaction of another substance also present (analogous to synaptase or emulsine), exactly as the oils of bitter almonds and mustard are formed.

By the action of barytic water, this gualtherine yields a new acid, the gualtheric acid. Unfortunately the author has not made use of elementary analysis.

Oil of Betula lenta.—This oil smells and tastes like gualtheria oil, has a specific gravity of 1·173, becomes reddened by the action of the atmosphere, but loses the colour on distillation; is but little soluble in water, in all proportions in alcohol and æther; produces a purple-red colour with protosalts of iron; forms crystalline compounds immediately with solution of potash, soda, baryta and oxide of lead; these yield the oil unaltered when treated with dilute sulphuric acid; when treated with excess of potash, salicylic acid is formed; it is slowly dissolved by ammonia, and the solution deposits crystals of salicylamide. Crystalline products are obtained by the action of chlorine and bromine, hydrochloric and bromic acids being

evolved. The chlorine compound, crystallized in four-sided prisms, yields by distillation with iodide, cyanide, and sulphuret of potassium, corresponding compounds. The sulphur compound is a yellow disagreeable-smelling oil, which becomes crystalline after a time. Hot nitric acid gives with the oil a crystallizable product, possessing acid properties; its combinations with bases detonate by heat. Concentrated sulphuric acid causes the formation of salicylic acid.

Gualtherine.—If the powdered dry bark be treated with alcohol of 0·805, the exhausted residue gives off no more odour on treatment with water; but when the tincture was evaporated, and a portion of the extract mixed with the exhausted bark, the odour was immediately produced, and by distillation, a product which reddened the protosalts of iron was obtained.

When the alcoholic extract is treated with water, tannic acid, sugar and colouring matter removed by digestion with hydrated oxide of lead, the filtered, almost colourless solution evaporated, the gummy residue treated with alcohol (0·800), and the solution allowed to evaporate spontaneously, a syrupy liquid was obtained, which could not be crystallized. Æther does not remove anything from it. This liquid, which evidently contains the gualtherine combined with a body which prevents its crystallization, is inodorous and of a slightly bitter taste; it may be obtained as a gummy mass when evaporated to dryness. The gualtherine remains undecomposed at 300°, at 400° it becomes perfectly black; it is combustible, leaving a copious carbonaceous residue. On destructive distillation, it yields a yellow oil, which resembles gualtheria oil contaminated with an empyreumatic matter, and leaves a carbonaceous residue.

In concentrated sulphuric acid the gualtherine dissolves, with the production of a red colour and the evolution of the odour of gualtheria oil. By distillation with dilute muriatic and sulphuric acid, oil of gualtheria is likewise formed. But if the gualtherine is dissolved in dilute nitric acid and distilled, instead of the oil, needles soluble in alcohol are found in the receiver, probably formed from the action of the vapours of the acid on those of the oil. Fuming nitric acid produces a yellow solution, from which water separates yellowish crystals. Ammonia has scarcely any action on gualtherine; by boiling with hydrated oxide of lead, a small quantity of a lead compound is obtained, which however contains no gualtherine. If an ammoniacal solution of gualtherine is precipitated with basic acetate of lead, a lead compound is obtained, from which no oil is evolved when treated with sulphuric acid, and which therefore contains no undecomposed gualtherine. Probably in this instance the same acid is formed, as is obtained when gualtherine is treated with the fixed alkalies and earths.

Gualtheric acid is best obtained when gualtherine is boiled with barytic water, the excess of baryta removed by carbonic acid, the solution filtered and carefully decomposed by dilute sulphuric acid. The solution filtered from the sulphate of baryta leaves a gummy mass on evaporation. If this be boiled with carbonate of lead, the solution filtered and decomposed by sulphuretted hydrogen,

on subsequent evaporation the acid is obtained as a colourless crystalline substance, readily soluble in alcohol and water, less so in æther, forming uncrystallizable salts with bases, and yielding gualtheria oil when distilled with dilute sulphuric acid.

The substance analogous to emulsine contained in the exhausted bark, the author could not isolate. It is insoluble in alcohol and water. By heating to 100° and treating with dilute solution of potash, the property of forming oil from gualtherine is lost.—*Pharm. Central.-Blatt. No.* 30.

OPIANIC ÆTHER.

M. Wöhler states that this compound cannot be obtained by saturating an alcoholic solution of opianic acid with hydrochloric acid gas. On evaporating the solution the acid crystallizes unchanged; it is, however, very readily formed, when sulphurous acid is passed into a hot alcoholic solution of opianic acid; a certain quantity is even produced when the acid instead of being dissolved is merely in suspension in the alcohol; the alcoholic solution after concentration and cooling, yields crystals of the æther; it has the form of small prisms either fasciculated or globular. It is inodorous, its taste slightly bitter, and insoluble in water; when heated under water, it melts at about 212° into a colourless liquid which sinks to the bottom of the vessel; on cooling, it becomes a white, opake, crystalline mass; it contracts extremely on solidifying, when melted without water; it solidifies into radiated white masses resembling navellite [wavellite?]; if its melting point be exceeded, it does not become solid again for a very long time; it may be sublimed between two plates of glass; when heated in a distilling apparatus, it creeps up the sides of the vessel, and thus apparently distils, without being really volatilized; it sustains a high temperature without decomposing; its vapour has but little odour, and burns with a sooty flame.

When long heated in water it gradually dissolves and is converted into alcohol and opianic acid. Solution of potash converts it into alcohol and opianate of potash. Cold caustic ammonia does not alter it.

By analysis it yielded—

	Experiments.		Equivalents.	Calculation.
	I.	II.		
Carbon	60·23	60·77	24	60·80
Hydrogen ..	5·70	5·84	13	5·47
Oxygen	34·07	33·39	10	33·73
	100·	100·		100·

Its formula is $C^{20} H^8 O^7 + C^4 H^5 O$.—*Ann. de Ch. et de Phys.*, Octobre 1844.

ON METALLIC ACIDS.   BY M. FREMY.

The author remarks, that he has already stated that some metallic oxides, which have hitherto escaped the notice of chemists, are capable of forming metallic acids, and that certain compounds of metals and oxygen, which have hitherto been considered as indifferent oxides, are true metallic acids.

Alumina.—This substance combines, as is well known, readily with the alkalies ; these salts are called aluminates. Nature presents us with compounds in which alumina is united with lime, magnesia, or oxide of lead, as in spinelle ruby, gahnite, and plomb gomme. Thus alumina may act the part both of an acid and a base.

The salts of alumina have been examined by a great number of chemists, but hitherto the aluminates have been incompletely studied ; and the author determined to ascertain the precise capacity of saturation of aluminic acid, by analysing a neutral crystallized aluminate.

Aluminates.

Aluminate of Potash.—This salt may be prepared either by dissolving alumina precipitated by carbonate of ammonia in potash, or by fusing in a silver crucible anhydrous alumina with excess of potash. If, in both cases, the solution be evaporated under the receiver of the air-pump, the liquor when sufficiently concentrated readily deposits hard brilliant crystals of aluminate of potash. This salt always retains a certain quantity of alkaline solution ; it may be fused and crystallized a second time.

Aluminate of potash is white, very soluble in water, but insoluble in alcohol ; it has a caustic taste and an alkaline reaction ; a certain quantity of water decomposes it, precipitates alumina nearly pure, and retains in solution a very alkaline aluminate.

M. Chevreul has observed, that when the acid of a salt is insoluble and the base soluble, the salt is often decomposed by a solvent into an acid salt, which is precipitated, and an alkaline one which remains in solution ; water effects a similar decomposition with aluminate of potash. The aluminate of potash having been dried *in vacuo*, was submitted to analysis ; this was effected by dissolving the aluminate of potash in a small quantity of water, and adding excess of sulphuric acid, and then precipitating the alumina by carbonate of ammonia, afterwards evaporating the filtered liquor, and weighing the sulphate of potash, which was ascertained to be neutral and entirely soluble in water.

100 parts submitted to analysis yielded—

Alumina....	40·6
Potash	37·5
Water	21·2
	99·3

Representing the composition of aluminate of potash by the formula $Al^2 O^3$, $KO + 3HO$, theory gives the following numbers :—

Alumina....	40·9
Potash	37·5
Water	21·6

It will be seen that by this analysis the salt must be considered as neutral, containing one equivalent of alumina and one of potash, and that in the neutral aluminates, the relation between the oxygen of the acid and that of the base is as 3 to 1.

In order to prepare a well-defined aluminate of potash of the above-described composition, the salt ought to be purified by solution in

a little water, or what is still better, in a weakly alkaline liquor, and crystallized; without this precaution, the salt which crystallizes is partly decomposed, retains always a certain quantity of acid aluminate, and when subjected to analysis it gives more alumina than is indicated by theory.

When water is expelled from aluminate of potash by heat, it appears to be partially decomposed, for when treated with water a part of the salt is found to be insoluble. Other salts formed by metallic acids undergo similar decomposition by heat.

Aluminate of soda is formed in the same way as aluminate of potash, but crystallizes less readily. Other aluminates are all insoluble; they are formed by double decomposition, and possess but little interest.

Iron and Ferric Acid.

M. Fremy remarks, that protoxide of iron is soluble in ammonia, and forms a combination which has not hitherto been examined; but this oxide does not combine with the fixed alkalies; when a solution of a protosalt of iron is precipitated by a concentrated solution of potash, and the liquor is boiled, it disengages a considerable quantity of hydrogen; the protoxide of iron is then converted into black oxide; the experiments of MM. Liebig and Wöhler have shown the facility with which protoxide of iron decomposes water.

Peroxide of iron does not appear to combine with the alkalies in definite proportion. When a persalt of iron is precipitated by excess of potash, the hydrate of the peroxide always retains a certain quantity of potash; the proportion of alkali combined is variable, and never occurs in simple relation to the oxide of iron.

If however the two oxides of iron do not combine directly with the alkalies, the author observes, that he has found by superoxidizing the sesquioxide of iron that it was converted into a compound which has all the properties of an acid, and to which he has given the name of ferric acid.

Thus then, iron, like manganese, chromium, tin, and several other metals, forms at first *energetic oxides*, and by superoxidizement yields a metallic acid.—*Ann. de Ch. et de Phys.*, Novembre 1844.

ON A NEW CHLORIDE AND OXIDE OF CHROMIUM.
BY M. EUG. PELIGOT.

It is well known that when a current of chlorine is passed over a mixture of oxide of chromium and charcoal, that a chloride is obtained which sublimes in fine violet-coloured scales; this compound corresponds to the sesquioxide of chromium, $Cr^2 O^3$; its composition M. Peligot states from recent experiments to be represented by $Cr^2 Cl^3$.

This is not however the only chloride formed in this operation; it is preceded by another chloride which appears to have escaped the notice of chemists, and which is in very white silky crystals, usually mixed with charcoal and oxide of chromium; these crystals become green immediately on exposure to the air, and quickly become a green solution; they absorb both oxygen and moisture at the same time

from the atmosphere. The analyses which the author has performed of this chloride leave no doubt as to its nature; it is composed of one equivalent each of metal and chloride, Cr Cl. It consequently corresponds to a degree of oxidation of chromium as yet unknown, CrO.

This same substance is formed when a current of hydrogen is passed over green chloride of chromium heated to low redness; hydrochloric acid is evolved, and a white crystalline mass remains; when operating at a higher temperature, this mass fuses, and on cooling presents a fibrous texture.

Protochloride of chromium prepared by either of these methods has the following properties:—it dissolves immediately in water; if the water is aërated, and if air be present during solution, the liquor has a green colour, but it is blue when the influence of oxygen is prevented. M. Peligot states that he is acquainted with no substance which is more rapidly altered than this by the contact of oxygen; so that in order to ascertain its properties it is necessary to operate with water deprived of air by ebullition, and in an atmosphere of carbonic acid gas.

The green solution resulting from the action of air and water on the protochloride of chromium, possesses the singular property of dissolving a considerable quantity of the violet chloride of chromium, with the disengagement of much heat; this latter body, when pure, is entirely insoluble in water and in acids; this property of the white chloride, which accompanies the violet chloride when prepared in the usual way, has thrown much uncertainty on the true characters of this latter body, which some authors consider as very soluble in water, and as producing a green solution, while others with reason dispute its solubility.

When protochloride of chromium has been prepared with the sesquichloride and hydrogen, it dissolves in water with the evolution of hydrogen; this decomposition of water, which is not considerable, seems to indicate the existence of a subchloride, which would result, like the subchloride of uranium, from the long-continued action of the hydrogen on the protochloride.

The blue solution of protochloride of chromium gives with potash a brown precipitate, which is probably a hydrate of the protoxide, corresponding to the chloride; ammonia gives a similar precipitate; an excess of ammonia gives a blue solution, which by exposure to the air becomes violet, and eventually red; monosulphuret of potassium precipitates the protochloride of chromium black.

On pouring a solution of acetate of potash or soda into the blue solution of chloride of chromium, small red transparent crystals are immediately formed, which rapidly collect at the bottom of the vessel. These crystals are spoiled by a few seconds' exposure to the air; it is however possible to obtain them in a pure state, by a process hereafter to be described, and when dry they have the aspect of protoxide of copper.

These crystals are the acetate of protoxide of chromium, the composition of which, taking the mean of four analyses which differed but little, is represented by the formula $C^4 H^3 O^3$, CrO, HO. A double sulphate of potash and protoxide of chromium gave the following as

the formula of its composition, $SO^3 KO + SO^3$, $CrO + 6HO$. The author has not yet determined the composition of the protoxide of chromium, but is engaged in ascertaining it.—*Journ. de Pharm. et de Ch.*, Nov. 1844.

ACETATE OF IRON AS A REMEDY FOR ARSENICAL PREPARATIONS.

The hydrated peroxide of iron is of admitted efficacy in cases of poisoning with uncombined arsenious or arsenic acid; but according to the experiments of Duflos, it is quite ineffectual when these acids are combined with bases, as for example, Fowler's solution, or arsenite of potash, or arseniate of potash, which last is frequently employed in calico-printing, and is consequently easily procured.

In the uncertainty in which the practitioner may be placed as to whether the arsenical poison be free or combined, it is very important to administer the oxide of iron in such a form as to produce a favourable result : to this end it is convenient to employ the peracetate of iron.

This compound may be prepared by adding to the hydrated peroxide of iron obtained from the decomposition of four parts of liquid perchloride of iron, three parts of acetic acid of density 1·06, and sufficient water to make up 16 parts.

This liquid, which is a solution of peracetate of iron with excess of base, precipitates arsenious and arsenic acid from all their solutions, either free, or combined with any base; 500 grains of it are sufficient to decompose 1880 grains of Fowler's solution.

This result demonstrates that liquid peracetate of iron merits preference in cases of poisoning by arsenical compounds. It should be remembered that its power is more rapid in proportion as it is more diluted with water; besides which, large dilution prevents all irritating action of the acetic acid set free.—*Journ. de Ch. Méd.*, Novembre 1844.

[The mode of preparing the above compound is not very clearly stated, for the strength of the liquid perchloride of iron is not given. It may be presumed that peracetate of iron mixed with peroxide is to be employed; the degree of excess of the latter is of little consequence, and the strength of the peracetate is limited by the employment of four parts of acetic acid of 1·06 made up to 16 parts with water, and this, it appears, is to be largely diluted before exhibited. —EDIT.]

ON A NEW CHARACTER OF THE SALTS OF LIME AND MAGNESIA.

M. Marchand remarks, that all treatises on chemistry agree in stating that the salts of lime and magnesia are not precipitated by the ferrocyanide of potassium. The ferrocyanides of calcium and magnesium being very soluble in water, also induced the belief that the characters stated by authors were correct; this however is not the case, for when ferrocyanide of potassium is added to even weak solutions of salts of lime or magnesia, an abundant precipitate of a slightly yellowish-white colour soon begins to deposit, and it adheres strongly to the sides of the vessel. The only precaution to be taken in producing the effect, is to take care that the solutions are per-

fectly neutral, because the precipitate is soluble even in weak acid. When heat is employed the precipitation takes place immediately.

The ferrocyanides of calcium and magnesium being, as already stated, very soluble in water, it became important to determine the nature of the precipitated salts; for this purpose a portion of the lime precipitate, which had been dried at $158°$ F., was treated with nitric acid and boiled in it till no trace of prussian blue remained; the liquor being evaporated to dryness and the residue analysed in the usual way, gave as results that the salt of lime was composed of

One atom of protocyanide of iron..	669·12	or	27·75
One ...	cyanide of calcium ..	585·93 ...	24·25
One ...	cyanide of potassium .	819·83 ...	34
Three ...	water	337·44 ...	14
		2412·32	100·

It results from this composition, that this salt is represented by the formula ($FeCy^2$, $CaCy^2$, $KCy^2 + 3H^2O$), and may be considered as a double ferrocyanide of potassium and calcium, or as a hydrated triple cyanide of iron, calcium and potassium; the salt is remarkable on account of the cyanogen being combined in equal proportions, and with equivalent quantities of potassium, calcium and iron, with a quantity of water sufficient to convert the cyanides into hydrocyanates.—*Journ. de Chim. Médicale*, Octobre 1844.

METEOROLOGICAL OBSERVATIONS FOR OCTOBER 1844.

Chiswick.—October 1. Clear : cloudy. 2. Densely clouded : clear. 3. Overcast : boisterous. 4. Clear and very fine : cloudy. 5. Very fine : rain. 6. Overcast : clear and fine : overcast. 7. Very fine : frosty. 8. Foggy : very fine. 9. Cloudy : boisterous at night. 10. Cloudy and mild : clear. 11. Heavy dew : very fine. 12. Foggy : very fine : showery. 13. Overcast : lightning at night. 14. Showery : rain. 15. Heavy shower : constant heavy rain at night. 16. Showery : clear and fine. 17, 18. Fine. 19. Slight haze : cloudy. 20. Fine. 21. Overcast : hazy. 22. Rain : cloudy : clear and frosty. 23. Dense fog : showery. 24. Cloudy : heavy rain. 25. Rain : densely clouded. 26, 27. Overcast and fine. 28. Foggy. 29. Overcast : rain. 30. Drizzly : cold haze. 31. Foggy : fine : clear.—Mean temperature of the month $0°·76$ below the average.

Boston.—Oct. 1. Fine. 2. Cloudy and stormy : rain early A.M. : rain A.M. 3. Windy. 4. Fine. 5. Cloudy : rain P.M. 6—8. Fine. 9. Cloudy : rain A.M. 10. Cloudy : rain P.M. 11, 12. Fine. 13, 14. Fine : rain P.M. 15. Cloudy : rain P.M. 16. Cloudy. 17. Cloudy : rain P.M. 18. Cloudy. 19. Fine. 20. Cloudy. 21. Cloudy : rain P.M. 22. Cloudy. 23. Foggy : beautiful halo round the moon quarter past eight P.M. 24, 25. Cloudy : rain P.M. 26. Rain. 27. Cloudy. 28. Fine : rain P.M. 29. Fine. 30. Cloudy : rain A.M. 31. Fine.

Sandwick Manse, Orkney.—Oct. 1—3. Showers. 4, 5. Bright : showers. 6. Shower : aurora : showers. 7. Bright : clear. 8. Cloudy : drops. 9. Cloudy. 10. Bright : cloudy. 11. Bright : clear. 12, 13. Bright : cloudy. 14. Drizzle : damp. 15. Rain : cloudy. 16. Rain : showers. 17. Shower : rain. 18. Showers : 19. Frost : cloudy : shower. 20. Clear : fine : aurora. 21. Frost : fine. 22. Clear. 23. Clear : cloudy. 24. Bright : clear. 25. Fine : clear : aurora. 26. Fine : clear. 27. Cloudy. 28. Cloudy : clear. 29. Bright : clear. 30, 31. Cloudy.

Applegarth Manse, Dumfries-shire.—Oct. 1. Fair. 2, 3. High wind and rain. 4. Rain P.M. 5. Fair, but cloudy. 6. Showers. 7. Fair and fine. 8. Fair, but dull. 9. High wind and rain. 10, 11. Showers. 12. Rain A.M. 13. Cloudy and threatening. 14. Showery. 15—17. Fair and fine. 18. Fair and fine : frost P.M. 19, 20. Showers : thunder : hail. 21. Fair and fine : frost. 22—24. Fair and fine. 25. Showery. 26. Fair and fine. 27, 28. Fair, but cloudy : frost. 29. Slight rain. 30. Rain P.M. 31. Fair, but cloudy.

Meteorological Observations made by Mr. Thompson at the Garden of the Horticultural Society at Chiswick, *near London; by Mr. Veall, at* Boston; *by the Rev. W. Dunbar, at Applegarth Manse,* Dumfries-shire; *and by the Rev. C. Clouston, at Sandwick Manse,* Orkney.

Days of Month 1844. Oct.	Barometer Chiswick Max.	Chiswick Min.	Boston 8½ a.m.	Dumfries-shire 9 a.m.	Dumfries-shire 9 p.m.	Orkney, Sandwick. 9¾ a.m.	Orkney, Sandwick. 8½ p.m.	Thermometer Chiswick Max.	Chiswick Min.	Boston. 8½ a.m.	Dumfries-shire Max.	Dumfries-shire Min.	Orkney, Sandwick. 9 a.m.	Orkney, Sandwick. 8½ p.m.	Wind Chiswick. 1 p.m.	Boston.	Dumfries-shire.	Orkney, Sandwick.	Rain Chiswick.	Boston.	Dumfries-shire.	Orkney, Sandwick.
1.	30·109	29·962	29·47	29·80	29·70	29·61	29·48	70	45	52	69	50	50	50½	sw.	sw.	sw.	w.				·76
2.	29·795	29·561	29·03	29·21	29·55	28·97	29·37	70	47	56	59	48½	53	49	w.	w.	w.	wnw.		·11		·43
3.	29·830	29·758	29·15	29·39	29·57	29·18	29·33	71	41	57	60	51	52	47½	sw.	w.	wnw.	w.		·02		·56
4. C	29·959	29·871	29·38	29·75	29·65	29·67	29·65	69	46	53	57	45½	48	46	sw.	w.	w.	w.	·22			·50
5.	29·802	29·686	29·18	29·52	29·63	29·59	29·58	67	39	57	55	45	46	44	sw.	calm	w.	w.				·11
6.	29·986	29·896	29·44	29·65	29·62	29·52	29·47	63	40	47	55	46	46½	45½	sw.	w.	wsw.	w.		·10		
7.	29·997	29·872	29·35	29·76	29·82	29·76	29·87	62	29	48·5	53	35½	45	39	nw.	calm	wnw.	nw.			0·72	·31
8.	29·946	29·568	29·46	29·68	29·35	29·59	29·46	61	41	42·5	51	32	47½	49	s.	se.	se.	s.				·24
9.	29·291	29·025	28·84	28·91	28·85	29·27	29·23	63	53	49·5	53	46	51	50	se.	s.	ese.	se.	·01	·04		
10.	29·440	29·194	28·74	29·48	29·15	29·01	29·13	63	35	56	58½	49	54	52	sw.	w.	ese.	se.	·07	·02		·03
11. ●	29·741	29·623	29·11	29·50	29·50	29·28	29·42	68	39	50	54	49	52	51	sw.	calm	ssw.	sse.	·03		1·40	
12.	29·724	29·540	29·18	29·13	29·27	29·49	29·41	64	55	52	58	40½	51	51½	s.	calm	ssw.	s.	·33			
13.	29·409	29·217	28·90	28·97	29·00	29·27	29·26	65	45	58	60	53	54	52	sw.	sw.	ssw.	se.	·42	·02		
14.	29·322	28·987	28·80	28·81	28·89	29·17	29·12	65	44	52	56	48	52½	51	s.	w.	e.	e.	1·04	·15		·10
15.	29·018	28·940	28·70	28·90	28·80	29·05	28·86	63	46	44	55	44	49½	48½	sw.	calm	e.	ne.	·02	·06		·72
16.	29·096	28·940	28·52	29·14	29·00	28·87	29·14	60	43	48	57	41	49	47	w.	calm	sw.	n.				·38
17.	29·450	29·232	28·74	29·53	29·31	29·28	29·44	59	37	48·5	53	43½	47	45	w.	w.	sw.	n.		·02		·33
18. ☽	29·779	29·618	29·15	29·40	29·61	29·61	29·65	56	33	39	51	42	42½	38	w.	calm	nw.	n.	·03			·05
19.	29·782	29·577	29·27	29·10	29·22	29·35	29·16	58	42	46	51	30	43	43	sw.	w.	sw.	s.	·03	·02		
20.	29·460	29·416	28·96	29·42	29·19	29·15	29·29	59	34	44	52½	38½	40½	39	sw.	calm	sw.	sse.	·64			
21.	29·601	29·510	29·14	29·75	29·66	29·58	29·79	59	43	49	52	34	43	41	e.	calm	ne.	nw.	·04	·68	0·20	
22.	29·897	29·766	29·27	29·74	29·77	29·83	29·72	59	30	37·5	51	30½	40½	40	w.	calm	se.	s.	·04			
23.	29·912	29·771	29·50	29·74	29·80	29·72	29·72	58	39	48	48	29	47	47	n.	calm	se.	se.	·88			
24.	29·706	29·671	29·38	29·74	29·90	29·74	29·92	52	46	49	50	43	46½	42	n.	calm	se.	sse.	·03	·59		
25.	29·853	29·760	29·40	29·88	30·14	30·07	30·12	52	40	52	53½	35	43	40	n.	calm	sw.	n.		·31		
26. ○	30·096	29·917	29·43	30·00	30·02	30·23	30·30	53	37	48·5	48½	35	44½	44½	ne.	calm	ne.	ne.		·10		
27.	30·252	30·237	29·79	30·24	30·09	30·38	30·40	53	29	41·5	51	43	45½	40½	ne.	calm	ne.	e.				
28.	30·209	30·129	29·79	30·18	29·88	30·38	30·33	47	31	48	51	45	46	43½	e.	calm	ne.	e.		·05	0·15	
29.	30·053	29·912	29·68	30·03	29·72	30·26	30·13	52	42	48	49	35	45½	47½	e.	calm	e.	ese.	·28			
30.	29·818	29·808	29·46	29·73	29·72	30·00	29·94	48	34	48	51	45	48½	48	e.	calm	e.	ese.	·02	·10		
31.	29·812	29·687	29·45	29·75	29·63	29·94	29·89	54	43		51	44½			e.	se.	ese.	se.				
Mean.	29·766	29·601	29·21	29·520	29·491	29·574	29·599	60·09	40·26	49·1	54·0	41·4	47·20	45·62					4·13	2·37	2·47	4·52

LXXVIII. *On Quaternions; or on a new System of Imaginaries in Algebra.* By Sir WILLIAM ROWAN HAMILTON, *LL.D., P.R.I.A., F.R.A.S., Hon. M. R. Soc. Ed. and Dub., Hon. or Corr. M. of the Royal or Imperial Academies of St. Petersburgh, Berlin, Turin, and Paris, Member of the American Academy of Arts and Sciences, and of other Scientific Societies at Home and Abroad, Andrews' Prof. of Astronomy in the University of Dublin, and Royal Astronomer of Ireland.*

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

I HAVE been induced to think that the account contained in the following letter, of the considerations which led me to conceive that theory of quaternions, a part of which you have done me the honour to publish in two recent Numbers (for July and October) of your Magazine, might not be without interest to some of your readers. Should you think proper to insert it, a public acknowledgement (very pleasing to my own feelings) will have been rendered, on the one hand to the Rev. Mr. Warren, whose work on the Geometrical Representation of the Square Roots of Negative Quantities (printed at Cambridge in 1828) long since attracted my attention and influenced my thoughts; and on the other hand to the gentleman (John T. Graves, Esq.), to whom the letter was addressed, and with whom I had been engaged, at intervals, for many years in a correspondence, very instructive and suggestive to me, on subjects connected therewith. Nor am I without hope that Mr. Graves may thus be led to communicate through you to mathematicians some of the extensions which he has made of results of mine, with some of those other speculations which are still more fully his own. On some future occasion I may perhaps be allowed to mention any other quarters from which I may be conscious of having derived more recent assistance, in my investigations on the

same mathematical subject, many of which are hitherto un-published.

<div style="text-align:center">I have the honour to be, Gentlemen,</div>

<div style="text-align:center">Your obedient Servant,</div>

<div style="text-align:center">WILLIAM ROWAN HAMILTON.</div>

Observatory of Trinity College, Dublin,
    November 20, 1844.

(*Copy of a*) *Letter from Sir William R. Hamilton to John T. Graves, Esq. on Quaternions.*

<div style="text-align:right">Observatory, October 17, 1843.</div>

MY DEAR GRAVES,—A very curious train of mathematical speculation occurred to me yesterday, which I cannot but hope will prove of interest to you. You know that I have long wished, and I believe that you have felt the same desire, to possess a Theory of Triplets, analogous to my published Theory of Couplets, and also to Mr. Warren's geometrical representation of imaginary quantities. Now I think that I discovered* yesterday a *theory of quaternions*, which includes such a theory of *triplets*†.

My train of thoughts was of this kind. Since $\sqrt{-1}$ is in a certain well-known sense, a line perpendicular to the line 1, it seemed natural that there should be some other imaginary to express a line perpendicular to both the former; and because the rotation from 1 to this also being doubled conducts to −1, it also ought to be a square root of negative unity, though not to be confounded with the former. Calling the old root, as the Germans often do, *i*, and the new one *j*, I inquired what laws ought to be assumed for multiplying together $a+ib+jc$ and $x+iy+jz$. It was natural to assume the product $= ax - by - cz + i(ay+bx) + j(az+cx) + ij(bz+cy)$; but what are we to do with ij? Shall it be of the form $\alpha + \beta i + \gamma j$? Its square would seem to be $= 1$, because $i^2 = j^2 = -1$; and this might tempt us to take $ij = 1$, or $ij = -1$; but with neither assumption shall we have the sum of the squares of the coefficients of 1, *i*, and *j* in the product $=$ to the product of the corresponding sums of squares in the factors. Take the simplest case of a product, namely, the case where it becomes a square; we have $a^2 - b^2 - c^2 + 2iab + 2jac + 2ijbc$ ∴ and $(a^2 - b^2 - c^2)^2 + (2ab)^2 + (2ac)^2 = (a^2 + b^2 + c^2)^2$; the condition

---

* The reader is requested to pardon this expression for the reason mentioned in another note.

† Prof. De Morgan has most obligingly given the writer a sketch of a Memoir on Triple Algebra, which he has lately presented to the Cambridge Philosophical Society, and which he is pleased to say was not begun till after the publication of the first part of the paper on Quaternions in this Magazine.

respecting the *moduli* is ∴ fulfilled, if we suppress the term involving *ij* altogether, and what is more, $a^2 - b^2 - c^2$, $2\,a\,b$, $2ac$ are precisely the coordinates of the *square-point*, so to speak, deduced from the point a, b, c, in space, by a slight extension of Mr. Warren's rule for points in a plane. [It is a long time since I read his book, but the grand features of his view cannot be forgotten.] In fact, if we double, in its own plane, the rotation from the positive semiaxis of x to the radius vector of the point a, b, c, we attain the direction of the radius vector drawn to $a^2 - b^2 - c^2$, $2\,a\,b$, $2\,a\,c$.

Behold me therefore tempted for a moment to fancy that $ij = 0$. But this seemed odd and uncomfortable, and I perceived that the same suppression of the term which was *de trop* might be attained by assuming what seemed to me less harsh, namely that $ji = -ij$. I made therefore $ij = k, ji = -k$, reserving to myself to inquire whether k was $= 0$ or not. For this purpose I next multiplied $a + ib + jc$, as a multiplier, into $x + ib + jc$, as a multiplicand, keeping still, as you see, the two factor lines in one common plane with the unit line. The result was $ax - b^2 - c^2 + i(a + x)b + j(a + x)c + k(bc - bc)$, in which the coefficient of k still vanishes; and $a\,x - b^2 - c^2$, $(a + x)\,b$, $(a + x)\,c$ are easily found to be the correct coordinates of the *product-point*, in the sense that the rotation from the unit line to the radius vector of a, b, c, being added in its own plane to the rotation from the same unit-line to the radius vector of the other factor-point x, b, c, conducts to the radius vector of the lately mentioned product-point; and that this latter radius vector is in length the product of the two former. Confirmation of $ij = -ji$; but no information yet of the value of k. Try boldly then the general product of two triplets, and seek whether the law of the moduli is satisfied when we suppress the k altogether. Is $(a^2 + b^2 + c^2)(x^2 + y^2 + z^2) = (ax - by - cz)^2 + (ay + bx)^2 + (a\,z + c\,x)^2$? No, the first member exceeds the second by $(bz - cy)^2$. But this is just the square of the coefficient of k, in the development of the product $(a + i\,b + j\,c)(x + iy + jz)$, if we grant that $ij = k, ji = -k$, as before. And here there dawned on me the notion that we must admit, in some sense, *a fourth dimension*[*] of space for the purpose of calculating with triplets; or transferring the paradox to algebra, must admit a *third* distinct imaginary symbol k, not to be confounded with either i or j, but equal to the product of

* The writer has this moment been informed (in a letter from a friend) that in the Cambridge Mathematical Journal for May last a paper on Analytical Geometry of n dimensions has been published by Mr. Cayley, but regrets that he does not yet know how far Mr. Cayley's views and his own may resemble or differ from each other.

2 K 2

the first as multiplier, and the second as multiplicand; and therefore was led to introduce *quaternions*, such as $a + ib + jc + kd$, or (a, b, c, d).

I saw that we had probably $ik = -j$, because $ik = iij$, and $i^2 = -1$; and that in like manner we might expect to find $kj = ijj = -i$; from which I thought it likely that $ki = j$, $jk = i$, because it seemed likely that if $ji = -ij$, we should have also $kj = -jk$, $ik = -ki$. And since *the order of multiplication of these imaginaries is not indifferent*, we cannot infer that k^2, or $ij\,ij$, is $= +1$, because $i^2 \times j^2 = -1 \times -1 = +1$. It is more likely that $k^2 = ij\,ij = -iijj = -1$. And in fact this last assumption is necessary, if we would conform the multiplication of quaternions to the law of the multiplication of moduli. For multiplying $a + ib + jc + kd$ as multiplier into $a' + ib' + jc' + kd'$ as multiplicand, and granting that k^2 is real, we find for the real part of the product, $aa' - bb' - cc' + k^2 dd'$; and for the coefficient of k, $ad' + da' +$ terms arising from the multiplication of i and j together; in order then that $2aa'dd'$ may disappear in the expression of the square of the modulus of the product, by being added to $2k^2 aa'dd'$, it is necessary that we should have $k^2 = -1$.

My assumptions were now completed, namely,

(A.) $i^2 = j^2 = k^2 = -1$; $\quad ij = -ji = k$; $\quad jk = -kj = i$; $\quad ki = -ik = j$:

and with these I was obliged to assume that if

$$(a, b, c, d)\ (a', b', c', d') = (a'', b'', c'', d''),$$

then the four following equations of multiplication hold good:

(B.)
$$\begin{cases} a'' = aa' - bb' - cc' - dd'\,; \\ b'' = ab' + ba' + cd' - dc'\,; \\ c'' = ac' + ca' + db' - bd'\,; \\ d'' = ad' + da' + bc' - cb'. \end{cases}$$

But I considered it essential to try whether these equations were consistent with the law of moduli, namely,

$$a''^2 + b''^2 + c''^2 + d''^2 = (a^2 + b^2 + c^2 + d^2)\ (a'^2 + b'^2 + c'^2 + d'^2),$$

without which consistence being verified, I should have regarded the whole speculation as a failure. Judge then of my pleasure when, after a careful examination, I found that the whole twenty-four products not involving squares in the development of the sum of the squares of the four quadrinomials (B.) destroyed each other; and that the sixteen products involving squares were just those which arise otherwise from the multiplication of $a^2 + b^2 + c^2 + d^2$ and $a'^2 + b'^2 + c'^2 + d'^2$*.

* Mr. Graves has since pointed out to the writer that this theorem of ordinary algebra is not new, and has very elegantly extended it.

We have, then, this first law for the *multiplication of two qua-ternions* together, that the modulus of the product is equal to the product of the moduli of the factors.

Division of quaternions is easy. The equations (B.) of multiplication give

(C.) $\begin{cases} a' = (a^2 + b^2 + c^2 + d^2)^{-1} (a\,a'' + b\,b'' + c\,c'' + d\,d''); \\ b' = (a^2 + b^2 + c^2 + d^2)^{-1} (-b\,a'' + a\,b'' + d\,c'' - c\,d''); \\ c' = (a^2 + b^2 + c^2 + d^2)^{-1} (-c\,a'' + a\,c'' + b\,d'' - d\,b''); \\ d' = (a^2 + b^2 + c^2 + d^2)^{-1} (-d\,a'' + a\,d'' + c\,b'' - b\,c''). \end{cases}$

The modulus of the quotient is the quotient of the moduli. A quaternion divided by itself gives for quotient $(1, 0, 0, 0) = 1$.

Addition and subtraction require no notice.

Making

(D.) $\begin{cases} a = \mu \cos \varrho, \; b = \mu \sin \varrho \cos \phi, \; c = \mu \sin \varrho \sin \phi \cos \psi, \\ \qquad\qquad d = \mu \sin \varrho \sin \phi \sin \psi, \end{cases}$

I would call ϱ the *amplitude* of the quaternion; ϕ its *colatitude*; and ψ its *longitude*. The *modulus* is μ. Representing the three coefficients b, c, d of the imaginary triplet $ib + jc + kd$, by the rectangular coordinates of a point in space; $\mu \sin \varrho$, as being the radius vector of this point, might be called the *radius*, or perhaps the *length*, of the quaternion. We may speak of the *inclination* of one quaternion to another, and its cosine is

$$\cos \phi \cos \phi' + \sin \phi \sin \phi' \cos (\psi' - \psi) = \frac{b\,b' + c\,c' + d\,d'}{\mu\,\mu' \sin \varrho \sin \varrho'}.$$

If with the amplitudes ϱ, ϱ', ρ'' of any two factors and their product, as sides, we construct a spherical triangle, the angle opposite to the amplitude of the product will be the supplement of the inclination of the factors to each other; and the angle opposite to the amplitude of either factor will be the inclination of the other factor to the product*.

This *theorem of the spherical triangle*, combined with the *law of the moduli*, requires only besides the following *rule of rotation*, to decide on which side of the plane of the two factor-lines the product-line is found, in order to complete the geometrical construction of the equations (B.) of multiplication of quaternions. In whichever direction, to the right or to the left, the positive semiaxis of i must rotate round that of k in order to approach to that of j; in the same direction must the multiplicand line turn round the multiplier-line in order to approach the product-line (or the plane containing that product-line and the multiplier-line).

If the factor-lines coincide with each other in direction,

---

* An improved form of this theorem of the spherical triangle was communicated by the writer to the Royal Irish Academy in November 1843, and has been published in the Philosophical Magazine for July 1844.

the product-line coincides with each of them; and its amplitude is the sum of theirs; because the spherical triangle degenerates into a single arc, two angles vanishing and the third becoming equal to two right angles. If the amplitudes of the factors are also supplementary to each other, the amplitude of the product is π, and its sine vanishes; the product has therefore in this case *no length*, or radius, but becomes a *pure real*, and at the same time a negative quantity; because $\mu'' \sin \rho'' = 0$, $\mu'' \cos \rho'' = -\mu''$. The factors are in this case of the forms

$$(\mu \cos \rho, \mu \sin \rho \cos \phi, \mu \sin \rho \sin \phi \cos \psi, \mu \sin \rho \sin \phi \sin \psi),$$

and

$$(-\mu' \cos \rho, \mu' \sin \rho \cos \phi, \mu' \sin \rho \sin \phi \cos \psi, \mu' \sin \rho \sin \phi \sin \psi);$$

and their product is

$$(-\mu \, \mu', 0, 0, 0) = -\mu \, \mu'.$$

Making the factors equal, $\varrho = \dfrac{\pi}{2}$, $\mu' = \mu$, we find that

$$(0, \mu \cos \phi, \mu \sin \phi \cos \psi, \mu \sin \phi \sin \psi)^2 = -\mu^2.$$

The square of a pure imaginary is real and negative: the square root of a real negative is a pure imaginary, having a determined length, but a wholly undetermined direction. The square root of -1 is any one of the infinitely many pure imaginaries, of the form

$$\sqrt{-1} = i \cos \phi + j \sin \phi \cos \psi + k \sin \phi \sin \psi.$$

In general, however, the square root of a quaternion has only two values, which differ only in sign: for if

$$(a'', b'', c'', d'') = (a, b, c, d)^2,$$

then

$$a'' = a^2 - b^2 - c^2 - d^2, \; b'' = 2\,a\,b\; c'' = 2\,a\,c\; d'' = 2\,a\,d\,;$$

$$\sqrt{a''^2 + b''^2 + c''^2 + d''^2} = a^2 + b^2 + c^2 + d^2,$$

$$a^2 = \tfrac{1}{2}(a'' + \sqrt{a''^2 + b''^2 + c''^2 + d''^2})\,;$$

and a^2 cannot vanish, except in the case of the pure negative square, $b'' = c'' = d'' = 0$, $a'' < 0$.

Multiplication will be easy if we are familiar with the rules for the product of two pure imaginaries. This product is, by (B.),

$$(0,b,c,d)\,(0,b',c',d') = (-bb' - cc' - dd', cd' - dc', db' - bd', bc' - cb');$$

the product-line is perpendicular to the plane of the factors; its length is the product of their lengths multiplied by the sine of the angle between them: and the real part of the product, with its sign changed, is the same product of the lengths of the factors multiplied by the cosine of their inclination.

Finally, we may always decompose the latter problem into

these two others; to multiply two pure imaginaries which agree in direction, and to multiply two which are at right angles with each other. In the first case the product is a pure negative, equal to the product of the lengths or moduli with its sign changed. In the second case, the product is a pure imaginary of which the length is the product of the lengths of the factors, and which is perpendicular to both of them. The distinction between one such perpendicular and its opposite may be made by the rule of rotation already stated.

There seems to me to be something analogous to *polarized intensity* in the pure imaginary part; and to *unpolarized energy* (indifferent to direction) in the real part of a quaternion: and thus we have some slight glimpse of a future Calculus of Polarities. This is certainly very vague*, but I hope that most of what I have said above is clear and mathematical. Hoping that you may be tempted to pursue the view which has thus opened, I remain, with best wishes,

<div align="right">

Your sincere friend,

WILLIAM R. HAMILTON.

</div>

LXXIX. *Notice of some New Zealand and Antarctic Minerals†.*

THE recent voyages and travels in the southern portions of the globe, although they have not revealed any mineral wealth, have still contributed some facts with reference to the physical condition of these regions, which appear to deserve a passing notice. The following are the results of the analyses of some mineral specimens, conducted by pupils of Dr. R. D. Thomson in the Glasgow College Laboratory. The specimens were presented by Dr. Dieffenbach and Dr. Joseph D. Hooker.

1. *Phosphate of Iron, or Native Prussian Blue‡.*—For this mineral from New Zealand Dr. Thomson was indebted to Dr. Dieffenbach. The locality of the phosphate is thus described by that naturalist:—"Near the Sugar Loaf Islands, by the river Urenui, the cliffs on the shore were about 100 feet high. The

---

* A pen has been drawn across a clause or two in the foregoing copy of this letter, as having reference only to this guess of a future physical application. A postscript has also been suppressed, as being comparatively private. In other respects, it has been thought that no change ought to be made, if the letter were to be printed at all; and therefore the indulgence of the reader is requested for some expressions which have the air of attaching to the subject an importance greater than it may deserve.

† Communicated by the Philosophical Society of Glasgow, having been read January 31, 1844.

‡ [We think that this appellation ought to be discarded by mineralogists, as the mineral is really not analogous to the artificial pigment called prussian blue.]

lowest formation was a marly clay; twenty feet above the sea was a formation of wood, very little altered or carbonized and ten feet in thickness, but irregular; above that was a loamy soil. From the lowest formation was dug out the phosphate, found in small pieces or balls. The natives call it *puke poto.* It is highly esteemed, when washed, as a paint." Its composition was found by Mr. Robert Pattison to be—

Water	28·40
Organic matter . .	2·80
Silica	5·20
Phosphate of iron .	62·80
	99·20

2. *Siliceous incrustation from New Zealand.*—Dr. Dieffenbach describes the interior of this island as being filled with hot volcanic springs, which deposit siliceous matter abundantly. A specimen of this kind, having some affinities to chalcedony, was found by Mr. Pattison to have a specific gravity of 1·968, and to consist of—

Silica	77·35
Alumina	9·70
Peroxide of iron . .	3·72
Lime	1·54
Water	7·66
	99·98

3. *New Zealand Ochre.*—Dr. Dieffenbach informs us, that " at 2699 feet above the sea, on Mount Egmont, the natives obtain the best sort of *kokowai* in the bed of the river, which was for some distance quite yellow from a solution in its waters of this ochreous substance, which glazed the rocks with a metallic coating. This muddy ochre was dug out of swamps formed by the Waiwakaio; it was slowly dried at the fire, and by further burning and preparing a fine vermilion was obtained: when mixed with shark's oil it forms a durable paint for the natives' houses, canoes, and burying-places, and is also used to rub on their faces and bodies in battle, and for mourning. It is also considered a defence against muskitoes and sandflies." It has been analysed by Mr. George Aitken, and found to consist of—

	I.	II.
Peroxide of iron	59·56	64·36
Silica	14·56	13·92
Water . . .	20·20	
Vegetable matter	4·72	
Alumina . . .	a trace	
Lime	a trace	
	99·04	

When digested in muriatic acid it partially gelatinizes. It is a rich iron ore, as it contains upwards of 40 per cent. of metallic iron. Specific gravity 2·24.

4. *Obsidian from New Zealand and Ascension.*—These specimens were analysed by Mr. James Murdoch.

	New Zealand.		Ascension.
	I.	II.	
Silica	75·20		70·97
Alumina . . .	6·86	6·29	6·77
Peroxide of iron .	6·54	8·06	6·24
Lime . . . }	3·83		2·84
Magnesia . . }			1·77
Soda and potash .	7·57		11·41

The New Zealand specimen was brought from the Bay of Islands; its specific gravity was 2·386. These analyses approximate closely to the results obtained by Berthier in his examination of obsidian from Pasco in Columbia.

5. Mr. Murdoch analysed a zeolite from Kerguelen's Land, which proved to be a white stilbite.

LXXIX. *On the Evidence of the former Existence of Struthious Birds distinct from the Dodo in the islands near Mauritius. By* H. E. Strickland, *Esq.,* M.A.*

IT is well known that Leguat, a French Protestant refugee, who for more than two years (from 1691 to 1693) resided in the island of Rodriguez, near Mauritius, described a bird under the name of *le Solitaire,* which Latham considered to be allied to, but distinct from, the Dodo, and which Gmelin denominated *Didus solitarius.* Later authors have supposed Leguat's bird to be either altogether fictitious, or to be founded on an imperfect description of the true Dodo, *Didus ineptus,* Linn., of whose former existence in the island of Mauritius there is now no dispute. Considering, however, that Leguat was a man of education, and that the rest of his narrative bears intrinsic proofs of veracity, there is no reason to doubt the general accuracy of his description of the *Solitaire*; and if this be admitted, it follows that his bird was distinct, generically as well as specifically, from the Dodo.

The *Solitaire,* as described and figured by Leguat, must have differed from the Dodo in the following respects:—

1. The beak is stated to resemble that of a turkey, except in being rather more curved. Leguat's figure corresponds with this description, and exhibits a moderate-sized gallinaceous-formed beak, totally unlike that which we know the Dodo to have possessed.

2. The *Solitaire* is said to have had hardly any tail, whereas the Dodo was depicted with an arched tail, like that of the ostrich.

3. The *Solitaire* is said to be longer in the leg (" plus haut mon-

* From the Proceedings of the Zoological Society, Part xii. p. 77, having been read April 23, 1844.

tée") than a turkey, while the Dodo was a very short-legged bird, as shown by the specimens in the British and Oxford Museums.

4. The *Solitaire* carried its neck erect, and this member was said to be longer in proportion than that of a turkey. But the Dodo is depicted with a short, thick and curved neck, corresponding with the massive proportions of its head.

5. Though unable to fly, the wings of the *Solitaire* appear to have been more developed than in the Dodo, as they were enlarged at the end into a knob the size of a musket-ball, with which the bird attacked its enemies.

6. The female *Solitaire* is stated to have a kind of band (probably composed of feathers) at the upper part of the beak, resembling a widow's cap; but in the Dodo the whole face was naked.

It seems then sufficiently evident, that as late as the year 1693 the island of Rodriguez was inhabited by a large species of bird distinct from the Dodo of Mauritius, and now exterminated. This bird was unable to fly; and Leguat, who gives a minute description of its habits, mentions the remarkable circumstance that it lays one egg on a heap of palm-leaves a foot and a half high, a character which possibly indicates an affinity to *Talegalla* and the *Megapodiinæ* of Australia.

The *Solitaire* of Rodriguez seems not to have been mentioned by any other author than Leguat, and we may presume that the species was exterminated within a few years after his visit.

There is evidence however that other apterous birds of this anomalous class formerly existed in the adjacent island of Bourbon. In the library of the Zoological Society is a manuscript presented by that active naturalist the late C. Telfair, Esq., who during his residence in Mauritius collected many valuable scientific and historical documents. This MS. is entitled ' Journal et Relation des Voyages faits par le Sr D. B. aux îles Dauphine ou Madagascar et de Bourbon ou Mascarenne.' 1669. The author, who seems to have been a very intelligent observer, speaking of the birds of the island of Bourbon, has the following passage :—

" Oiseaux de terre et leurs noms.

" *Solitaires* : ces oiseaux sont nommés ainsi, parce qu'ils vont toujours seuls. Ils sont gros comme une grosse Oye, et ont le plumage blanc, noir à l'extrémité des ailes et de la queue. À la queue il y a des plumes approchantes de celles d'Autruche, ils ont le col long, et le bec fait comme celui des bécasses, mais plus gros, les jambes et pieds comme poulets d'Inde. Cet oiseau se prend à la course, ne volant que bien peu.

" *Oiseaux bleus*, gros comme les *Solitaires*, ont le plumage tout bleu, le bec et les pieds rouges, faits comme pieds de poules, ils ne volent point, mais ils courent extrêmement vite, tellement qu'un chien a peine d'en attraper à la course ; ils sont très bons."

The author then proceeds to describe the wild pigeons and other birds of Bourbon.

It appears then that about the year 1670 the island of Bourbon was inhabited by two species of Struthious birds, one of which was called

Solitaire, and the other *Oiseau bleu.* The *Solitaire* of Bourbon seems however to have been distinct from, though probably allied to, the bird of that name in Rodriguez. Its plumage is stated to have been white, with the wings and tail terminated with black, whereas Leguat describes the Rodriguez bird as grayish and brown. The Bourbon species further differed in having a tail similar to that of an ostrich, and in the beak being lengthened, " like that of a woodcock, but stouter," in which respect it must have resembled the Apteryx of New Zealand. The phrase " ne volant que bien peu " would seem to imply that the bird possessed some powers of flight, though possibly it may only mean that when hard pressed the bird aided its progress by flapping the wings, or by springing into the air for a short distance.

The *Oiseaux bleus* seem to have been a distinct species both from the Dodo and from the Solitaires of Bourbon and of Rodriguez, and to have been wholly unable to fly, but possessed, like the Apteryx, of great cursorial powers.

We are then justified in believing, from the relations of authors apparently deserving of credit, that the three contiguous islands of Mauritius, Bourbon and Rodriguez were formerly inhabited by at least four distinct species of birds, deprived, or nearly so, of the power of flight, and more nearly allied in structure to the Apteryx of New Zealand than to any other existing genus of birds. And if the account given by Cauche of a tridactylous and apterous bird in Mauritius, called *Oiseau de Nazarette,* be correct, we must believe in the former existence of a fifth species of the same anomalous family.

Nor need we be surprised at the supposition that the species allied to the Dodo may have been thus numerous, when we recollect that Prof. Owen has already shown that no less than five species of that ornithic wonder, the Dinornis, inhabited New Zealand at a very recent date, and were doubtless contemporaries of the still surviving Apteryx. Still less should we wonder at the speedy extinction of these birds after man took possession of the Mauritian archipelago. Confined to very small islands, unable to escape from their enemies by flight, and highly esteemed for food, they soon experienced the same fate as that of the Dinornis, a fate which will shortly overtake the unprotected Apteryx[*].

Having thus shown that there is good historical evidence of the former existence of several Struthious or *Didiform* birds in the Mauritian group of islands, we may inquire whether any actual remains of these deceased species are still attainable. On this point I must be content rather to excite inquiry than to supply information. Of the Dodo, as is well known, we possess an entire head, and the feet of two individuals ; but of the other birds above referred to, no relics

[*] It is probable that in 1693, when Leguat visited Mauritius, the Dodo had been extinct a considerable time. He makes no mention of any such bird, but remarks " L'île était autrefois toute remplie d'Oyes et de Canards sauvages, de Poules d'eau, de Gelinottes, de Tortues de mer et de terre ; *mais tout cela est devenu fort rare ;*" showing that at that period, when the Dutch had occupied Mauritius for nearly a century, civilization had made great inroads on the fauna of the island.

have yet been identified. M. Quoy, however, assured M. de Blain-
ville that the bones in the Paris Museum which Cuvier supposed to
belong to the Dodo, were brought, not from Mauritius, but from
Rodriguez; and it is therefore probable, as supposed by M. de Blain-
ville, that they may have belonged to the *Solitaire* of Leguat. There
are, too, certain bones from Rodriguez presented by Mr. Telfair to
this Society (Zool. Proceedings, Part I. p. 31); and in the Ander-
sonian Museum at Glasgow there are also some so-called " Dodo's
bones from Mauritius." All these materials should be submitted to
careful examination; and we may feel confident that if Prof. Owen,
who has so skilfully demonstrated the affinities of the Dinornis from
a few fragments of the skeleton, were to take these materials in hand,
he would soon deduce some valuable results, whether positive or
negative, from the investigation.

Much light also might probably be thrown on the subject if natu-
ralists residing in Mauritius, Bourbon and Rodriguez would endea-
vour to obtain further evidence. The alluvia of streams, the soil on
the floors of caverns, and even the ancient mounds of rubbish near
towns and villages, should be carefully searched, and every fragment
of bone preserved. We may hope that the success which has at-
tended such researches in New Zealand will stimulate the naturalists
of Mauritius to similar efforts, and that the *Solitaires* and *Oiseaux
bleus* will ere long, like the Dodo and the Dinornis, take their just
rank in our systems of ornithology.

LXXXI. *On the Inorganic Constituents of Plants.* By Drs. H. Will *and* R. Fresenius*.

SINCE the true value and importance of inorganic constitu-
ents for the existence of plants has been pointed out and
established by Professor Liebig, it has become an object of
high interest in vegetable physiology and agriculture, to pos-
sess an extended series of trustworthy analyses of the ashes
of different plants, and of different parts of plants, conducted
with reference to the points to which the Professor has di-
rected attention in his work, " Organic Chemistry in its appli-
cations to Agriculture and Physiology." We say trustworthy,
and thus specially directed analyses, since in those already at
command the requisite conditions are but partially supplied.

To the exemplary labours of De Saussure concerning vege-
tation, is due the undisputed merit of having first incited to a
knowledge of the importance of the inorganic constituents of
plants; he established the fact by means of numerous analyses
of vegetable ashes. The methods of chemical analysis, and
especially of the quantitative estimation of the more important
constituents of vegetable ashes (phosphoric acid for example),
were not however at that time such as to render the results of

* Communicated by the Chemical Society; having been read May 20,
1844.

De Saussure available for the determination of the functions fulfilled in plants by inorganic acids and bases.

Berthier* has also made a considerable number of analyses of the ashes of various woods; his only object however was to ascertain, if possible, what effects the ashes of the wood used as fuel in metallurgy would have upon those processes.

Berthier found by a comparison of his carefully obtained analytical results, that the nature of the ash depended to some extent upon the properties of soil; he observed that the composition of the ashes of the same kind of wood grown on different soils, differed considerably; he showed also that the ashes of different kinds of plants grown upon the same soil were dissimilar, and that plants which are identical or allied in kind, when grown upon the same soil, yield ashes, the compositions of which are also either allied or identical. Finally, Wiegmann and Polstorff†, by ascertaining the quantity of ashes of plants which have vegetated in sand and in artificial mould, have clearly shown that the increase of plants, their growth, and full development are inseparably connected with the organic materials contained in the earth, and that plants will not perfect their seeds if the earth is deficient in phosphates, &c.

All recorded observations clearly show that plants take up indiscriminately the soluble mineral constituents presented to them by the soil; and the results of ash-analyses as distinctly prove, that for the purpose of assimilation by their different parts and organs, they make a selection from those constituents; that the organism of vegetables, like that of animals, takes up and retains those substances which it requires.

It follows from what has been said, that in the analysis of ashes, a number of substances will be found which have not entered into the composition of any organ of the plant, substances which have been dissolved by the juices of the plant; and, since we have no means of safely separating them for the purposes of analysis, they will remain after the ignition mixed with the assimilable and assimilated mineral constituents of the plant.

Nature has provided in the seeds of plants, as in the eggs and milk of animals, all the substances which are necessary for the development of the young individual, so long as it is itself incapable of seeking sustenance externally. In the ashes of seeds we shall find therefore treasured up, and almost pure, the indispensable mineral food of the plants.

The examinations hitherto made of the ashes of seeds indicate that those of the *Cerealia*, the *Leguminosæ*, the *Cruciferæ*,

* *Ann. de Chimie et de Phys.* xxxii. p. 240.

† *Ueber die unorgan. Bestandtheile der Pflanzen; gekrönte Preisschrift.* Braunschweig, 1842.

and among arboreous plants, the *Coniferæ*, consist almost entirely of the alkaline and earthy phosphates. They do not effervesce with acids, they contain however variable quantities of silica and sulphates, but chlorides are either entirely wanting, or merely traces of them exist. The ashes of the seeds of the oak, of the chestnut and of the beech, and probably of many other trees, effervesce strongly with acids; besides phosphates they contain a considerable quantity of carbonates, which have resulted from the salts of vegetable acids contained in the seeds. The proportion of chlorides, sulphates and silica to the other constituents, is also in these ashes but small.

We conclude from these facts, that the salts of phosphoric acid are indispensable mineral constituents of the Cerealia, &c., and that the oak and the chestnut, &c. require, besides these, alkalies not united with mineral acids.

It would be very difficult at present accurately to distinguish the so-called essential and non-essential constituents of ashes. A plant requires for its different parts, and at different periods of its growth, a supply of mineral constituents, varying both in quantity and in quality. There is no ash-constituent which we can as yet decide to be entirely unessential. We can however distinguish those which have been assimilated by the plant and have formed a part of it from those which are within the plant, without having been assimilated by it, but await, as it were, their approximation in its future development and maturation. Among the latter we may class the alkaline chlorides and the sulphates, which are always present in vegetable ashes; they exist in the form of soluble compounds in the juices of every plant, and are doubtless indispensable to it, as yielding some of its elements; we have however as yet no reason for supposing that they themselves become constituents of the organs of the plant. According to Guibourt (*Journ. de Pharm.* xxvi. p. 264), the ashes of juicy saline plants contain only 17 per cent. of chlorides to 69 per cent. of carbonates. It would seem highly probable that the bases of salts of vegetable acids are derived from chlorides; the chlorides in this case suffer a decomposition, in which the electro-negative element plays but a subordinate part. These chlorides, which seem so necessary for saline plants, may probably be replaced by other compounds of the same basyles, provided they be not injurious to the plants, and that they be equally soluble with the chlorides, and consequently existing as largely in the juices of the plant.

We have further reason for supposing these constituents to be unessential, from the fact that their quantities from the same kind of plants are variable, without there being evidence that they are replaced by any other body. The quantity of sul-

phates found in an ash will, however, to some extent, be de-
pendent on the management of the preparation of the ash. If a
strong heat, with free access of air, be permitted, part if not all
of the sulphur contained in nitrogenized vegetable substances
will be converted into sulphuric acid during the ignition. On
the other hand, however, if the ash has not been heated suf-
ficiently long with free access of air, the addition of an acid
to a portion of it will cause the evolution of a not inconsider-
able quantity of sulphuretted hydrogen, the source of which has
been the deoxidized sulphates. In such a case, if it be wished
to make a quantitative analysis of the ash, it must be heated
with free access of air until the sulphurets are fully oxidated.

The carbonic acid, and the charcoal found in the ashes of
most vegetable substances (with the exception of some seeds),
are always to be viewed as accidental constituents, having their
origin in the operation of combustion ; their quantity also is
dependent both upon the quantity and quality of the bases
present, and upon the degree of heat which has been employed.

We now submit to view a list of the *inorganic* compounds
or bodies which have hitherto been found in the normal ashes
of vegetables. They are as follow :—

	Acids or bodies which replace them.
Bases.	
Potash.	Silicic acid.
Soda.	Phosphoric acid.
Lime.	Sulphuric acid.
Magnesia.	Carbonic acid.
Peroxide of iron.	Chlorine.
Oxide of manganese.	Iodine.
Alumina. ?	Bromine.
	Fluorine*.

With the exception of iodine, bromine, fluorine and oxide

* It has long been known that marine plants, and consequently their
ashes also, contain iodine and bromine. In some the quantity of iodine
is so considerable, that they are had recourse to in commerce for the pro-
duction of that element, as in Scotland, for example. The existence of
fluorine in some vegetable ashes, although extremely probable, has not
been proved until recently. Careful experiments, conducted under my own
superintendence, by Messrs. James, Müller and Blake severally, have shown
that the ashes of French barley, grown in Switzerland, contain very distinct
traces of it ; both straw and grain were employed. The presence of silica
in considerable quantity renders the detection of fluorine very difficult.
The process followed was that of H. Rose, which consists in fusing the ash
with carbonated alkali, dissolving in water, neutralizing with hydrochloric
acid, in a platinum capsule, adding ammonia in excess, after expelling the
carbonic acid by evaporation, and precipitating the filtered solution by
means of chloride of calcium. In this experiment not less than 10 to 12
grammes of ash should be employed. Fluorine occurs in the teeth and
bones of animals, having been derived by them from vegetable food ; it will
doubtless therefore exist still more abundantly in the ashes of plants : it

of manganese, the acids and bases enumerated above are found in almost all vegetable ashes. Several chemists have stated that alumina also is found in the ashes of plants. Berthier, on the other hand, did not find a trace of it in the ashes which he examined, and we have been unable to detect it in all carefully prepared ashes. De Saussure states that the ashes of the bilberry, the pine and the rose-laurel, contain respectively 17·5, 14·8 and 28·8 per cent. of alumina; it is evident however that he has mistaken the earthy phosphates for that substance, and when in subsequent analyses he determined the phosphates, he found no alumina, or merely a trace of it. The behaviour of alumina with solvents sufficiently proves that it cannot be taken up by plants. Pure alumina is equally insoluble in solutions of phosphoric and of carbonic acid; and though we find in almost all vegetable ashes phosphate of the peroxide of iron, which is so nearly allied to alumina in insolubility, it has probably been taken up by the plant as phosphate of the protoxide, which is soluble in a solution of carbonic acid. If the aluminous compounds, which are so abundant in the soil, were soluble, we should find them in all vegetable ashes; and even were the presence of alumina demonstrated in particular cases, we have sufficient reason for supposing that it, as well as the sand, which is frequently met with in the analyses of ashes, has been derived from dust, which has not been removed from the plant with sufficient care previously to the preparation of the ash.

Oxide of copper has also been found occasionally in vegetable ashes; Meissner and Saczeau observed it in those of the Cerealia. In some parts of Germany the seeds of the Cerealia are steeped in a solution of sulphate of copper before being sown, with a view of preventing the destruction of the crop by blight: and when oxide of copper is found in vegetable ashes, it may be attributed either to such treatment, or to the presence of cupreous minerals in the soil; it is always however to be looked upon as unessential or accidental, and not as a necessary constituent of the ashes of the plant.

The relative proportions of the constituents of vegetable ashes vary so much, according to the nature of the plant, or the part of the plant (and also according to the properties of the soil), that it has been attempted to classify plants by reference to the predominant constituent of their ashes. Such a classification will only be attainable when we can, with respect to a large number of analyses, ensure the necessary accuracy: for our immediate purpose, however, that of arranging

almost constantly accompanies phosphoric acid, and must therefore be almost as generally diffused on the earth's surface as that acid itself, which is so indispensable both for the vegetable and animal kingdoms.—H. WILL.

a method of quantitative ash-analysis, the ashes of vegetables can be generally classed according to the prevalence of some of their constituents.

All vegetable ashes can be arranged according to their composition in the three following groups:—

(a.) *Ashes rich in Alkaline and earthy Carbonates.*—To this group belong the ashes of woods, herbaceous plants and lichens, inasmuch as these abound in salts of organic acids.

(b.) *Ashes rich in Alkaline and earthy Phosphates.*—Almost all seeds yield ashes belonging to this division.

(c.) *Acids rich in Silica.*—The stems of the Grasses, the Equisetaceæ, &c., yield ashes of this kind.

It will be seen that this classification cannot be strict, and that besides the instances already known, many others will occur of members of the different classes assimilating. The ashes of the mistletoe (*Viscum album*), for example, form a link between the first and second groups, inasmuch as they contain considerable quantities both of carbonates and of phosphates; the same may be said of those of the seeds of the oak (*Quercus robur*) and of the chestnut (*Fagus castanea*). Again, the ashes of some seeds, as millet (*Millium sativum*), oats (*Avena sativa*), barley (*Hordeum vulgare*), contain so much silica that they might with equal propriety be placed in the third as in the second group.

The division of plants into potash, lime and silica plants is quite inadmissible, since it would be inconsistent with the beautiful law of substitution established by Professor Liebig. According to this law, the predominance of the one or the other of the above-named bases in the ashes of a plant depends upon the quantities of each existing in the soil. Tobacco would generally be considered as belonging to the lime group; the result of recent analyses, of great interest in reference to the law mentioned above, prove however that tobacco would equally belong to the potash group if grown upon a soil in which that base abounded.

We now proceed to describe a method of quantitative ash-analysis which, as yet, we have found to be the simplest, and therefore most to be depended upon. The details of the process comprise but little that is new to science, whilst a method, more or less similar, might easily have been arranged by any experienced chemist. We are nevertheless of opinion that circumstances justify its ample consideration. The subject is as yet but partially investigated, and there are doubtless chemists appreciating its importance, and desirous of entering upon the inquiry, to whom it will be a desideratum to be possessed of a method, into the correctness of which they need not themselves laboriously examine.

Method of Quantitative Ash-analysis.

I. *Preparation of the Ash.*—It is evident that the removal of all foreign matter from the vegetable substances selected is of the greatest importance: whether seeds, herbs or roots, they should be carefully examined and freed from adherent dirt before being burned. They must not be washed, however, since in that case some of the soluble salts would be extracted; for the same reason, plants which have been exposed in a withered or dead state, to rain or moist weather, should not be employed. Plants in a healthy and natural condition should always be selected, unless the object in view be the study of disease and its causes.

The information sought by means of ash-analysis, may have reference to the quantity and quality of the inorganic matter contained in a particular kind of plant, or part of a plant; or it may be desired by its aid to judge of the properties of the soil, as regards the presence in it of alkalies, alkaline earths and phosphates. If the latter be the object, a quantitative examination of the ashes of all the plants grown upon the soil will frequently suffice. A knowledge being once attained of the composition of the ashes, both of the cultivated plants and of the weeds, we shall be enabled to decide respecting the composition of the soil, and its suitableness for the production of each particular plant.

Woods, herbs and roots, after being thoroughly dried, should be burned upon a clean iron plate; but for leaves, fruits and seeds, it is more convenient to employ a hessian crucible, heated by means of burning charcoal, in which it is placed somewhat obliquely. The ash is occasionally very easily burned white, but some seeds require a more elevated temperature, to get rid of the charcoal which is intimately mixed with the phosphates; care should always be taken, however, to avoid heating so strongly as would cause the fusion or softening of the alkaline salts, should they abound, or it will be quite impossible to burn the charcoal, which in that case becomes inclosed in the mass. It has properly been observed by De Saussure, that the ashes are whitest when the access of air has not been impeded by stirring or shaking them together.

The ash thus prepared should, if necessary, be further heated to low redness in an open platinum capsule, over a spirit-lamp, with constant stirring; afterwards, and whilst still warm, it should be rubbed to a very fine powder, intimately mixed, and put into a well-closed bottle.

It is now determined, by means of a qualitative analysis, to which of the three groups the ash belongs. Its solubility in concentrated hydrochloric acid is first ascertained: this is in

most cases complete, unless the ash be obtained from the stems
of the grasses, which abound in silicic acid.

After the separation of the silica, the acid solution is treated
with acetate of ammonia, or it may be neutralized by means
of caustic ammonia, free acetic acid being then added ; in most
cases a yellowish-white gelatinous precipitate is formed, which
consists of phosphate of the peroxide of iron.   This precipi-
tate is collected upon a filter, and ammonia is added in excess
to the clear solution, by which a fresh precipitation will ge-
nerally be occasioned.   If a red precipitate be formed, it is
peroxide of iron; if, after standing some time excluded from
the air, no precipitation takes place, we conclude that the ash
contains no phosphate besides the one previously separated ;
but if a white precipitate be formed (phosphate of lime and
phosphate of magnesia), it is known that there is more phos-
phoric acid present than is required to combine with the per-
oxide of iron.   Nothing further is necessary for the completion
of the *qualitative* analysis, but to test for fluorine and oxide of
manganese, and, in the case of marine plants, for iodine and
bromine ; unless, indeed, it happen that the experimenter have
reason to suspect the presence of other substances in the ash.

For the determination of the chlorine (iodine and bromine)
and of the carbonic acid, separate portions of ash are always
employed, as also for that of the alkalies, in ashes belonging
to the third group.   All the other constituents are determined
in one quantity of ash, so that the silica has only to be sepa-
rated once.

A. *Analysis of Ashes rich in Carbonates and Phosphates.*

1. *Determination of the Silica of the Charcoal and of the*
Sand.—About 4 grammes of the ash, which has been found to
be soluble in hydrochloric acid, is treated with the concen-
trated acid in a matrass, held obliquely, so as to avoid the
loss of fluid during the evolution of carbonic acid ; gentle heat
is then applied, until it is evident that everything is dissolved,
excepting the carbonaceous and sandy particles.   The whole is
now carefully removed into a porcelain basin, evaporated to
dryness over a water-bath, and then heated somewhat more
strongly, as is usual in separating silica.   The mass when
cold is moistened with strong hydrochloric acid, is digested for
half an hour with a sufficient quantity of water, and is boiled,
after which the acid liquid is poured upon a stout filter which
has been previously dried at 100° C. and weighed.

The silica will remain upon the filter, and if the ash was
not perfectly white and pure, some sand and charcoal also.
The filter is washed and dried, and the substance carefully
removed from it into a platinum crucible without injury to the

paper. This is easily effected if the matters be perfectly dry, the paper in most cases only retaining so much as to be slightly coloured by the charcoal. The powder is now boiled for half an hour with pure potash ley (free from silica), by which the whole of the silica will be gradually dissolved, leaving the sand and the charcoal unacted upon. The insoluble matter is again collected on the same filter, washed, and dried at 100°C. until it no longer loses weight. The increase upon the weight of the dried filter is estimated as charcoal and sand.

The filtered solution, after the addition of hydrochloric acid in excess, evaporation to dryness, and treatment in the usual way, gives the quantity of silica.

The acid solution, originally filtered from the silica, sand and charcoal, after being well mixed, is divided into three, or more conveniently into four equal portions, one being reserved in case of misfortune with either of the other quantities. The division is best accomplished by means of an accurately graduated tube : the whole of the fluid is collected into the tube, the entire measure then representing the weight of ash experimented upon. The solution is now divided into three or four equal or known portions, the volume of each is noted, and they are distinguished by the letters a, b, c and d.

In (a) the peroxide of iron, oxide of manganese, and the alkaline earths are estimated.

In (b) the alkalies.

In (c) the phosphoric and sulphuric acids.

2. *Estimation of the Peroxide of Iron and of the Alkaline Earths.*—To the solution (a) ammonia is added until the precipitate which is thereby produced no longer entirely redissolves ; acetate of ammonia is next added, and sufficient acetic acid to render the solution strongly acid. From the form and appearance of the precipitate it can easily be judged whether it still contains phosphate of lime ; if this be the case, more acetic acid must be added. The yellowish-white precipitate which remains consists of phosphate of the peroxide of iron, $2Fe_2 O_3 + 3PO_3$. Its separation from the fluid is assisted by gently heating it, it is then well washed upon a filter with hot water, ignited and weighed *.

* We have submitted to careful analysis the white, or rather greenish-yellow precipitate, formed by mixing solutions of the common phosphate of soda and a persalt of iron. The formula of this compound, when dried without heat over sulphuric acid, is $2Fe_2 O_3 + 3PO_5 + 13Aq$. Of these 13 equivalents of water 6 go off at 100° C., 4 more at from 300° to 400°, but the remaining 3 are only expelled by strong ignition. The rational formula is therefore $3PO_5, 2Fe_2 O_3, 3HO + 4HO + 6Aq$. The numbers obtained by analysis were as follow :—

I. 1·8087 grm. of the substance dried over sulphuric acid lost at 100° C. 0·199 water=1·100 per cent., and 0·131 more=7·24 per cent. at 300° to 400°.

To the filtered solution neutral oxalate of ammonia is added as long as a precipitate is formed, and the quantity of lime is determined in the usual manner. When it has been shown by quantitative analysis, that besides phosphate of iron, the ash contains peroxide of iron or oxide of manganese (in which cases the presence of earthy phosphates is very rarely detected), the solution, previously to the separation of the lime, should be supersaturated with ammonia and precipitated by means of sulphuret of ammonium; the two oxides being afterwards separated according to the known methods.

If the ash under examination contained earthy phosphates, the solution filtered from the oxalate of lime will contain free acetic acid; if otherwise, ammonia will be free; it is next somewhat concentrated, rendered ammoniacal, treated with a solution of phosphate of soda, and separated from the precipitate, which is estimated as phosphate of magnesia.

3. *Estimation of the Alkalies.*—The solution (*b*) is treated with baryta water until it gives an alkaline reaction, it is then gently heated and filtered. By this means we get rid of all the sulphuric acid, phosphoric acid and peroxide of iron, as well as the magnesia and most of the lime. The precipitate is washed upon a filter as long as the liquid renders turbid a solution of nitrate of silver; the solution is next warmed, treated with caustic ammonia and carbonate of ammonia, and allowed to stand until the precipitate becomes heavy and granular. The whole is now filtered, and the solid matter is washed, after which the solution is evaporated to dryness, and the residue heated to redness in a platinum capsule to expel the ammoniacal salts. What remains consists of the chloride of potassium or sodium, or more generally of a mixture of the two. The weight being noted, a little water is added, which mostly leaves undissolved a trace of magnesia; this is collected on a filter, its quantity is subtracted from that

II. 1·4787 of the salt dried at 300° to 400° C. lost by ignition in one experiment 7·13 per cent., and in second 6·62 per cent. water; the mean is 6·87 per cent. = 5·61 per cent. of the salt not dried at an elevated temperature.

III. 0·409 of the salt dried at 300° (= 0·5015 dried over sulphuric acid) yielded 0·158 peroxide of iron = 31·5 per cent.

The composition of the salt is therefore—

		In 100 parts.	
		Calculated.	Found.
2Fe₂O₃ . . .	1957	32·09	31·50
3PO₅	2678	43·92	
3HO	337	5·54	5·61
4HO	450	7·38	7·24
6Aq.	675	11·07	11·00
		100·00	

of the supposed alkaline chlorides and added to that of the magnesia as previously ascertained. The quantity of potash is determined by means of chloride of platinum in the usual way; and that of the soda is calculated from that of the chloride of sodium, indicated by deducting the weight of the chloride of potassium from that of the mixed alkaline chlorides.

4. *Estimation of the Sulphuric and Phosphoric Acids.*—From the acidulous solution (*c*), the sulphuric acid is first separated by means of chloride of barium. The filtered liquid is nearly neutralized with ammonia, acetate of ammonia is added, and then a solution of perchloride of iron, until the liquid begins to turn red, owing to the formation of acetate of iron. Care must here be taken that sufficient acetate of ammonia be added to convert the whole of the chlorine of the perchloride into sal-ammoniac. The solution is now boiled until it is colourless, all the iron being then precipitated. The precipitate, after being washed with hot water, consists of phosphates of iron and a quantity of basic acetate of iron. It is dried, ignited in a platinum crucible, treated with a few drops of nitric acid, re-ignited and weighed. It is next digested with concentrated hydrochloric acid, which speedily dissolves it. The solution is diluted with hot water mixed with tartaric acid, and ammonia is added until the yellowish-white precipitate which is at first formed disappears. A clear solution, but of a dingy green colour, is thus obtained, from which the iron is precipitated by means of sulphuret of ammonia. The precipitate and supernatant fluid are digested together in a warm place until the latter loses its green tinge and is of a clear yellow colour, resembling that of the sulphuret of ammonia. It is now rapidly filtered, access of air being excluded, and the precipitate is washed with hot water containing a little sulphuret of ammonia, until a drop of the filtered liquid, dried upon a platinum spatula and ignited, no longer indicates an acid reaction. The sulphuret of iron is dissolved from the filter by means of hot and dilute hydrochloric acid, the solution is boiled, treated with a few drops of nitric acid to peroxidize the iron, and ammonia is then added in excess. The precipitate is pure peroxide of iron, whose weight, deducted from that of the basic phosphate, gives the quantity of phosphoric acid. It is essential that the perchloride of iron used in this experiment be quite free from sulphuric acid.

5. *Estimation of the Chlorine.*—A fresh portion of ash (about 1 gramme) is weighed out, exhausted by means of hot water slightly acidulated with nitric acid, and the solution is precipitated with nitrate of silver. If the ash contain appreciable quantities of iodine or bromine, these bodies will be found in

the precipitated silver-salt; for their quantitative estimation, however, a larger portion of ash must be employed.

6. *Estimation of the Carbonic Acid.*—The quantity of carbonic acid is represented by the loss sustained on treatment of a known weight of ash with an acid; and we have elsewhere described the most convenient method and apparatus for the experiment.

B. *Analysis of Ashes abounding in Silica.*

Ashes of this kind, as we have already observed, are generally only partially soluble in acids; their alkalies must therefore be determined in a separate portion of ash. The chlorine and carbonic acid are determined in the same manner as when the ash is entirely soluble in acids. The quantity of chlorine found in ashes of this class is however probably always some-what less than it should be, since the alkaline chlorides, when ignited with silica and carbon, undergo a partial decomposition.

1. *Estimation of the Silica, &c.*—Pure potash or soda ley is poured upon about 4 grammes of the ash and evaporated to dryness in a platinum or silver dish. The silicic acid compounds are by this treatment dissolved, leaving the sand unaffected. The heat should not be so great as to fuse the mass, or some of the charcoal will be oxidated at the expense of the water of the hydrated alkali. Dilute hydrochloric acid is poured upon the mass, the whole evaporated, and the silica, charcoal, &c. determined in the manner already described. The acidulous solution filtered from the insoluble matter is divided into two parts; one is employed for the determination of the sulphuric and phosphoric acids, and the other for that of the peroxide of iron and the alkaline earths, by the methods detailed above.

2. *Estimation of the Alkalies.*—A second portion of ash (say 3 grammes) is ignited in a crucible of platinum or silver with four times its weight of hydrated barytes, the acid solution which remains after separating the silica, &c. is precipitated successively with barytes water, and carbonate of ammonia, the alkalies being then obtained in the state of chlorides.

C. *Determination of the Quantity of Ash.*

It is often important, even indispensable, to ascertain the entire quantity of inorganic constituents contained in a plant or part of a plant. A certain quantity of fixed matter is annually removed from cultivated land in the crop, and it is a desideratum to have a scale for the reparation of this loss by means of manures. The chemist who is engaged with ash-analysis should therefore acquire information as to the quantity which will be

taken from a given surface by the crop.   Rational agriculturists and woodreeves are always in possession of details of this
kind, which are generally of sufficient accuracy for the ends
to be answered by them.

The vegetable substance under examination should be dried
over a water-bath at 100° C. until it no longer loses weight ; if
however any of its constituents be volatile, the drying is best
conducted over sulphuric acid, without the application of heat.
The quantity of substance to be burned to ash will depend
upon the quantity of fixed matter it contains.   Of herbs and
seeds, which are rich in inorganic constituents, 2 to 3 grammes
will be sufficient ; of woods, however, which frequently contain
only 0·2 per cent. of fixed matter, ten or more times that
quantity must be taken.   The combustion succeeds best in a
thin platinum crucible ; it is at first covered and heated only
gently, but afterwards the lid is removed and a stronger heat
employed until the whole of the charcoal is consumed.   The
ashes of seeds which do not effervesce with acids may be
moistened with nitric acid and re-ignited, when they will very
speedily be turned white.

We now submit a number of ash-analyses which were conducted according to the methods we have described.   The
first are those of ten kinds of tobacco, which were sent to
Professor Liebig for the purpose by the director of the Imperial Austrian tobacco manufactory in Vienna.   All these
plants were cultivated in Hungary, upon land which had never
been manured, and upon which the same plant had been grown
for a considerable length of time.

The specimens 1, 2 and 3 were from the Debreczyner, No.
4 from the Banat, and Nos. 5 to 10 inclusive from the Fünfkirchner district.   With the exception of Nos. 7, 8, 9 and 10,
the leaves reached us in a sound and perfectly healthy state,
still partially retaining their green colour.   Differences were
however clearly indicated by the varying size and greater or
less luxuriance of the specimens.

In addition to the above we analysed the ashes of the grain
of red and of white wheat (*Triticum vulgare*), of the seed and
of the straw of rye (*Secale Cereale*), of peas (*Pisum sativum*),
of the wood of the apple-tree (*Pyrus malus*), of the mistletoe
(*Viscum album*) from the same tree, and of lichens (*Parmelia
prunastri, fraxinea, parietina, furfuracea*) : all were grown
in the neighbourhood of Giessen.

TABULATED RESULTS OF THE ANALYSES OF THE ASHES OF TOBACCO.

(A.) *Actual results of the analyses of* 100 *parts of Ash.*

	Debreczyner.			Banat.	Fünfkirchner.					
	I.	II.	III.	IV.	V.	VI.	VII. Dried at the tempe-rature of a room.	VIII. Powder of leaves.	IX. Powder of stalk.	X. Powder of leaves.
					1840.	1841.		1841.		1839.
Potash	23·33	22·90	22·63	14·48	5·77	13·62	6·28	6·01	7·35	6·55
Soda	1·81	......	......	......	......	0·19				
Lime	22·19	18·51	25·29	22·16	30·08	30·99	31·98	31·74	27·09	27·44
Magnesia	5·79	5·79	5·93	12·51	9·80	7·71	9·46	10·01	10·31	7·46
Chloride of sodium	0·73	4·44	7·59	9·07	2·27	2·46	2·99	2·06	4·38	1·51
Chloride of po-tassium	......	......	3·98	3·12	6·00	......	2·88	2·10	2·05	1·74
Phosphate of per-oxide of iron	7·04	4·50	5·19	5·41	4·27	3·02	3·37	4·32	5·19	3·69
Phosphate of lime	......	......	......	......	......	4·16				
Sulphate of lime .	5·16	4·50	5·22	8·04	5·60	3·92	4·34	3·94	6·46	3·00
Silica	14·16	13·73	5·35	4·75	6·54	3·58	3·59	4·03	5·72	7·04
Carbonic acid......	10·45	9·11	14·78	15·90	15·09	21·95	22·69	17·08	17·30	17·49
Charcoal and sand	10·31	13·50	4·73	6·46	13·72	8·04	11·62	19·36	13·80	23·75
	100·97	97·28	100·69	101·90	99·14	99·64	99·20	100·65	99·74	99·70

(B.) *Product of* 100 *parts of Ash, after deducting the carbonic acid, the charcoal and the sand.*

	I.	II.	III.	IV.	V.	VI.	VII.	VIII.	IX.	X.
Potash	29·08	30·67	27·88	18·20	8·20	19·55	9·68	9·36	10·37	11·21
Soda	2·26	......	......	......	......	0·27				
Lime	27·67	24·79	31·16	27·86	42·77	44·49	49·28	49·44	39·53	47·03
Magnesia	7·22	8·57	7·31	15·73	13·93	11·07	14·58	15·59	15·04	12·77
Chloride of sodium	0·91	5·95	9·34	11·41	3·22	3·54	4·61	3·20	6·39	2·58
Chloride of po-tassium	......	......	4·90	3·92	8·53	......	4·44	3·27	2·99	2·97
Phosphate of per-oxide of iron	8·78	6·03	6·39	6·80	6·07	4·34	5·19	6·72	7·56	6·30
Phosphate of lime	......	......	......	......	......	5·97				
Sulphate of lime...	6·43	5·60	6·43	10·11	7·96	5·63	6·68	6·14	9·42	5·11
Silica	17·65	18·39	6·59	5·97	9·32	5·14	5·54	6·28	8·34	12·03
	100·00	100·00	100·00	100·00	100·00	100·00	100·00	100·00	100·00	100·00
Oxygen in the bases united to organic acids .	16·06	15·47	16·31	16·99	18·78	20·15	21·09	21·50	18·75	20·05
Per-centage of ashes obtained from the dry leaves	18·9	22·2	24·5	19·8	23·0	21·08	23·28	23·28	22·83	27·36

(A.) *Actual Results of the analyses of* 100 *parts of the ashes of*

	Wheat.		Rye.		Peas.	Apple-tree.		
	Red. Grains.	White. Grains.	Grain.	Straw.		Wood.	Mistle-toe.	Lichen.
Potash	20·80	30·17	31·89	17·03	39·25	13·67	35·32	} 10·07
Soda	15·01	.. ...	4·33	.. ...	3·96	0·32	......	
Lime	1·83	2·76	2·84	8·98	5·87	45·19	19·40	10·33
Magnesia	9·12	12·08	9·86	2·39	6·41	5·30	9·59	5·33
Peroxide of iron...	1·29	0·28	0·80	1·35	1·04			
Phosphate of per-oxide of iron	......	......	......	......	......	1·71	1·83	14·56
Phosphoric acid...	46·91	43·89	46·03	3·80	34·23	2·95	16·56	..
Chloride of sodium	......	......	trace.	0·56	3·69	0·32	1·02	
Chloride of po-tassium	......	......	......	0·25				
Sulphuric acid ...	......	......	1·42	0·81	4·89	0·66	1·41	
Silica	0·15	......	0·17	63·89	......	0·93	1·62	
Carbonic acid......	......	......	......	......	......	24·18	13·09	4·64
Charcoal and sand	4·89	9·03	2·66	0·94	1·38	2·03	0·54	49·73
	100·00	98·21	100·00	100·00	100·72	97·26	100·38	94·66*

* The remaining constituents were not estimated.

(B.) *Per-centage composition after deducting the carbonic acid, the charcoal and the sand.*

	Wheat.		Rye.		Peas.	Apple-tree.		
	Red. Grains.	White. Grains.	Grain.	Straw.		Wood.	Mistle-toe.	Lichen.
Potash	21·87	33·84	32·76	17·19	39·51	19·24	40·71	
Soda	15·75	......	4·45	.....	3·98	0·45	......	
Lime	1·93	3·09	2·92	9·06	5·91	63·60	22·37	
Magnesia	9·60	13·54	10·13	2·41	6·43	7·46	11·06	
Peroxide of iron...	1·36	0·31	0·82	1·36	1·05			
Phosphate of per-oxide of iron	......	......	......	......	......	2·41	2·11	
Phosphoric acid...	49·32	49·21	47·29	3·82	34·50	4·15	19·09	
Chloride of sodium	......	......	......	0·57	3·71	0·45	1·17	
Chloride of po-tassium	......	......	......	0·26				
Sulphuric acid ...	0·17	......	1·46	0·83	4·91	0·93	1·62	
Silica	......	......	0·17	64·50	......	1·31	1·87	
	100·00	100·00	100·00	100·00	100·00	100·00	100·00	
Quantity of oxy-gen in the bases of the phosphates ...	12·35	11·92	11·97	......	11·18			

On comparing these analyses of tobacco-ash, it is at once seen that plants of the same kind, grown upon the same soil, yield ashes, the composition of which differs but little. This is very evident with respect to those of the Debreczyner leaves (1, 2 and 3). The proportion of bases combined with organic acids is, so far as their oxygen is concerned, very nearly the same in each case. The quantity of lime in the ashes of the Banat leaves is almost the same as in those of the Debreczyner leaves, whilst the quantity of potash in the former is scarcely two-thirds as great as in the latter; the quantity of magnesia, however, the equivalent of which is so small, is nearly doubled. The ashes of the Fünfkirchner leaves exhibit a still wider difference; in four instances the quantity of potash is scarcely one-third of that in the Debreczyner leaves; in place of this potash, however, we find an increase in the quantity of lime, amounting to nearly 50 per cent. Again, the quantity of magnesia is the same as in the ashes of the Banat leaves, whilst the lime in the latter only amounted to 27 per cent.

It is evident that these differences depend entirely upon the variable quantities of the several bases contained in the soils. In these differences we recognise moreover a beautiful confirmation of the law of mutual replacement of bases laid down by Professor Liebig. Supposing this law to be subject to no modification, and considering the accuracy of the analyses, we should have expected that in equal weights of the various ashes (the abnormal matters being deducted), the quantities of oxygen united to these bases would have been more nearly identical. In the case of plants containing much sap, this difference in the quantities of oxygen contained in the bases united to organic acids, is easily accounted for; partly by the reactions which we have already stated take place during the ignition of the ash, and partly by the influence which, as in the case of tobacco, the presence of nitrates, according to their quantity, will exert upon the amount of carbonates found in the ash.

The ashes of seeds, so far as their composition is known, consist, like those of the blood of man and other animals, principally of the phosphates of the alkalies and alkaline earths. The curious fact, moreover, has lately been observed, that the phosphoric acid is not in all seeds combined with the same number of equivalents of fixed base. Analyses conducted by ourselves and under our superintendence, show that peas, beans, horse-beans, &c., that is, the Leguminosæ, and the seeds of the Coniferæ (the pines), &c., contain tribasic phosphates ($PO_5 + 3MO$). The seeds of the Cerealia (rye, wheat, millet, buck-wheat, and also hemp and linseed) contain, on the other

hand, bibasic phosphates ($PO_5 + 2MO$). This relation is maintained also when the seeds are grown in different localities. The solution of ashes containing tribasic phosphates gives a yellow precipitate with nitrate of silver, even if made immediately after ignition, whilst that of those containing bibasic phosphates gives a pure white precipitate. The quantities of oxygen contained in the bases of these seeds (deducting the sulphates and chlorides) are so nearly alike, that it is evident that the law of replacement of bases applies also to the phosphates. The following analyses demonstrate this fact.

I. *Analyses of Seed-Ashes containing tribasic phosphates, sometimes also mixed with bibasic phosphates.*

The analyses marked thus * were conducted under the superintendence of Dr. Will.

	Peas.			
	Fresenius and Will (Giessen).	Bichon (Holland).	Thon (Kurhessen).	Boussingault (Bechelbronn).
Potash	39·51	34·19	35·20	36·31
Soda	3·98	12·86	10·32	2·56
Lime	5·91	2·46	2·70	10·39
Magnesia	6·43	8·60	6·91	12·24
Peroxide of iron..............	1·05	0·96	1·94	
Phosphoric acid...............	34·50	34·57	34·01	31·00
Sulphuric acid	4·91	3·56	4·28	4·83
Chlorine (or chlor. sodium)	3·71	0·31	2·56	1·13
Silica	......	0·25	0·29	1·54
Oxygen in the bases	11·18	12·23	11·44	13·51

	Horse-Beans (Vicia faba).			Beans (Phaseolus vulgare).	
	Bichon (Holland).	Boussing. (Alsace).	Buchner (Giessen).*	Thon (Kurh.).	Boussing. (Alsace).
Potash	20·82	47·14	32·71	21·71	51·36
Soda	19·06	......	12·75	21·07	
Lime	7·26	5·33	4·72	5·38	6·07
Magnesia	8·87	8·98	6·13	7·35	12·03
Peroxide of iron..........	1·03	......	0·66	0·34	
Phosphoric acid..........	37·94	35·67	39·11	35·33	28·53
Sulphuric acid	1·34	1·66	......	2·28	1·36
Chlorine..................	1·48	0·71	......	3·32†	0·10
Silica	2·46	0·51	0·47	1·48	1·05
Oxygen in the bases	13·56	12·42	12·69	13·04	14·73

† Chloride of sodium.

	Poleck.*			Muller.
	Scotch Fir.	Silver Fir.	Pinus Picea.	Brassica Napus.
Potash	22·37	18·57	21·75	21·34
Soda	1·26	5·78	6·76	5·26
Lime	1·86	1·54	1·54	14·63
Magnesia	12·57	16·79	16·79	11·96
Oxide of iron	2·51	1·31	1·31	2·84
Phosphoric acid............	45·95	39·65	39·65	41·68
Chloride of sodium	......	......	0·57	
Silica	......	0·57	11·71	1·52
Sulphuric acid	10·44	11·71	......	· 0·77
Oxygen in the bases	12·94	......	14·60	14·17

II. *Analyses of Seed-Ashes with bibasic phosphates.*

	Wheat.				
	Fresenius and Will (Giessen).		Bichon (Holland).	Thon (Solz in K.).	Boussingault (Bechelbronn).
	Red.	White.			
Potash	21·87	33·84	6·43	24·17	30·12
Soda	15·75	......	27·79	10·34	
Lime	1·93	3·09	3·91	3·01	3·00
Magnesia	9·60	13·54	12·98	13·57	16·26
Peroxide of iron...........	1·36	0·31	0·50	0·52	
Phosphoric acid............	49·32	49·21	46·14	45·53	48·30
Sulphuric acid	0·17	......	0·27	......	1·01
Chlorine (chlor. sodium)					
Silica	......	......	0·42	1·91	1·31
Quantity of oxygen in the bases	12·35	11·92	14·20	12·98	11·84

	Rye.		Buckwheat.	Maize.
	Fresenius and Will (Giessen).	Bichon (Cleve).	Bichon (Cleve).	Letellier (Bechelbronn).
Potash	32·76	11·43	8·74 ⎫	30·8 & loss.
Soda	4·45	18·89	20·10 ⎭	
Lime	2·92	7·05	6·66	1·3 ...
Magnesia	10·13	10·57	10·38	17·0 ...
Peroxide of iron	0·82	1·90	1·05	
Phosphoric acid..............	47·29	51·81	50·07	50·1 ...
Sulphuric acid	1·46	0·51	2·16	
Silica	0·17	0·69	0·69	0·8 ...
Quantity of oxygen in the bases	11·97	12·30	11·96	

| | Bichon. | Poleck.* | Leuchtweiss.* | | Boussing. |
	Barley.	Millet.	Hemp-seed	Linseed.	Oats.
Potash	3·91	9·58	21·67	25·85	12·9
Soda	16·79	1·31	0·66	0·71	......
Lime	3·36	0·61	26·63	25·27	3·7
Magnesia	10·05	7·66	1·00	0·22	7·7
Peroxide of iron...	1·93	0·63	0·77	3·67	1·3
Phosphoric acid...	40·63	18·19	34·72	40·11	14·9
Sulphate of lime...	......	0·60	0·18	1·70	
Chloride of sodium	......	1·43	0·09	1·55	0·5
Silica	21·99	59·63	14·04	0·92	53·3
Sulphuric acid ...	0·26	......	......	......	1·0
Carbon	......	......	......	......	4·7
Oxygen of the } bases .... }	11·70	......	13·88	12·76	14·95

III. *Additional Analyses of Seed-Ashes.*

	Kleinschmidt, Acorn.*	Buch. Saintfoin.*
Potash	64·64	6·75
Soda	......	20·33
Lime	4·89	
Magnesia	5·57	8·57
Peroxide of iron......		
Phosphoric acid......	15·62	54·89†
Sulphate of lime ...	4·73	2·87
Chloride of sodium...	0·98	2·18
Silica	0·96	1·10
Phosphate of per-} oxide of iron ... }	2·61	3·31 .

	Fruit of the Horse-chestnut. De Saussure.
Carbonate of potash	51
Phosphate of potash	28
Chloride of potassium and } sulphate of potash ...... }	3
Earthy phosphates	12
Silica	0·5
Metallic oxides	0·25
Loss	5·25

It is of great importance that analyses should be made of the ashes of the most widely differing plants, and also of those of the same kind of plant grown on very different kinds of soil. This can only be accomplished, however, by the persevering cooperation of many chemists. The examples given above show how beautiful and fertile in their consequences are the results obtained by ash-analyses; and we hope that by means of the methods herein described, the work will be rendered somewhat more easy.

The difference in the solubility of the nitrogenous ingredients of the Leguminosæ, as regards their vegetable casein, and the nitrogenous ingredients of the Cereals, in respect of vegetable fibrin, arises evidently from the greater amount of alkaline bases contained in the seeds of the Leguminosæ, in comparison to the quantity of phosphoric acid, to which they have immediate reference. This larger quantity of base causes the

† Phosphate of lime.

solubility of the Leguminosæ in water, whilst the vegetable fibrin of the Cereals is insoluble therein. There is therefore ground for supposing that the phosphates are also bibasic in the Leguminosæ as well as in the Cereals; the third atom of the base would be consequently combined with the vegetable casein, and the tribasic phosphate be formed during the incineration.

The great quantity of sulphuric acid (from 3·5 to 4·9 per cent.) contained in the ashes of peas is remarkable in comparison to that of other seeds : in the ashes of beans it amounts to 2 per cent., in the ashes of wheat and rye to only 0·5 per cent., and is frequently altogether wanting. It is probable that the cause arises from the small quantity of phosphoric acid present, whilst the sulphur of the nitrogenous ingredients is more easily retained as sulphuric acid by the alkaline bases during the incineration. If fresh-prepared ashes from seed containing tribasic phosphates are exposed to the air, they take up carbonic acid. This is the reason why Boussingault[*] found in the ashes of horse-beans one per cent., and in that of the common beans 3·3 per cent. of carbonic acid.

Upon comparing the ashes of the wood of the apple-tree with that of the mistletoe growing from it, the difference in the quantity of potash and phosphoric acid contained in each is immediately evident : for whilst the ashes of the apple-tree wood contain only 19 per cent. of potash and 4 per cent. of phosphoric acid, we find that the ashes of the wood of the mistletoe contain double the quantity of potash and five times the quantity of phosphoric acid.

The mistletoe therefore appears to perform upon the tree the same functions (as far as regards the inorganic ingredients) as the fruit, in absorbing from the sap the phosphates. This is no doubt the cause of the injury produced on trees by parasitic plants. Further analytical investigations with this and other parasites will show whether this can be considered as a general law.

LXXXII. *On the Purification of the Soluble Salts of Manganese from Iron.* By ALEXANDER KEMP[†].

THE commercial oxide of manganese commonly contains several impurities, but invariably a quantity of iron. The separation of this iron from the salts of manganese becomes a matter of some importance, and consequently a number of processes have been given by chemists at different times for this purpose.

* *Economie Rurale,* t. ii. p. 327.

† Communicated by the Author. On a subject closely related to that of this paper, see Phil. Mag. S. 1. vol. lix. p. 86.

The chloride of manganese being one of the salts from which a considerable number of the other compounds of the metal are obtained, I shall first describe the method for its purification.

The common black oxide of manganese having been boiled in muriatic acid, so as to obtain the solution as neutral as possible, it is to be filtered when cold, and then to have added to it a solution of carbonate of ammonia, by which the iron will be precipitated as a brownish powder; and provided care be taken not to add an excess, the whole of the manganese will remain in solution; this of course can be easily ascertained by testing the filtered liquid with ferrocyanide of potassium. The liquid will now hold in solution the chloride of manganese, along with a small quantity of chloride of ammonium, which latter substance is to be got rid of by evaporation to dryness, and exposure to a temperature of $500°$ or $600°$.

The sulphate of manganese may be obtained free from iron by following the same mode of purification.

For the preparation of the carbonate of manganese the process may be slightly modified, and a less expensive salt substituted for the carbonate of ammonia. The solution of impure chloride or sulphate of manganese is to have carefully added to it a solution of carbonate of soda, until on testing the liquid it is found that all the iron has been thrown down; it is next to be filtered, when a further addition of carbonate of soda will cause the precipitation of the carbonate of manganese free from iron. As the solution of the sulphate of manganese is now used in dyeing, this, I think, would be found a more œconomical process than that at present followed for its preparation. The carbonate of soda might be used for the precipitation of the iron instead of the carbonate of ammonia; the small quantity of sulphate of soda left in the liquid would not be likely to interfere with the use of the salt in dyeing.

LXXXIII. *Abstract of a Letter from* H. B. Leeson, *M.D.,*
on the Preparation of Fluoride of Iodine[*].

THE fluoride of iodine, of which I presented a specimen to the Chemical Society on the 1st of April, was prepared by passing the gas generated from 1 part of peroxide of manganese, 3 of pure fluor spar, and 6 of concentrated sulphuric acid through water, in which the iodine was diffused (contained in a glass vessel) until the whole of the iodine was taken up. A leaden retort and conducting tube was made use of.

[*] Communicated by the Chemical Society; having been read May 20, 1844.

The leaden tube did not appear to be acted upon, neither could any trace of lead be detected in the product, which gradually deposits in crystalline scales, very similar in appearance to those of iodide of lead. Fluoride of bromine was prepared in a similar manner, but did not yield a crystalline deposit, being very soluble in water. The latter answers extremely well for the Daguerreotype, giving instantaneous pictures, but I cannot at present decide that it is superior to bromine alone.

I hope when the Society reassembles to furnish them with a more detailed account of these compounds.

LXXXIV. *On the Propagation of Waves in a Resisted Fluid ; with a new Explanation of the Dispersion and Absorption of Light, and other Optical Phænomena. By the* Rev. M. O'Brien, *Professor of Natural Philosophy and Astronomy in King's College, London, and late Fellow of Caius College, Cambridge.*

[The subject resumed from p. 334.]

To the Editors of the Philosophical Magazine and Journal.
Gentlemen,

17. IN a paper which you did me the honour to print in your Magazine for November, I attempted to show that the general facts of the dispersion and absorption of light may be accounted for by supposing that the vibrations of the æthereal fluid are *resisted* by the particles of matter, the forces of resistance brought into play being of the same nature as those which act upon a particle moving in an ordinary resisting medium. I made no hypothesis whatever respecting the *law* of the resistance experienced by the æthereal fluid, assuming nothing more than simply the existence of that resistance; and I showed that, in the important and really general case of circularly polarized light (for all kinds of polarized light may be formed by compounding circularly polarized rays), the equations of motion may be completely integrated without our knowing anything respecting the law of resistance, and that thus the following formulæ may be obtained, namely,

$$\xi = a\,e^{-hz}\cos(n\,t - k\,z), \qquad \eta = a\,e^{-hz}\sin(n\,t - k\,z), \quad (1.)$$

where h and k are given by the equations

$$2\,A\,k^2 = n\left\{\sqrt{\left(n+\frac{N}{v}\right)^2 + \left(\frac{T}{v}\right)^2} + \left(n+\frac{N}{v}\right)\right\}$$
$$2\,A\,h^2 = n\left\{\sqrt{\left(n+\frac{N}{v}\right)^2 + \left(\frac{T}{v}\right)^2} - \left(n+\frac{N}{v}\right)\right\}$$
(2.)

N and T representing the forces of resistance upon, and v the velocity of any element of the æthereal fluid.

I also showed that, supposing n and k to be positive, h is essentially positive, which is a result of considerable import-ance.

I now proceed to examine what consequences result from certain suppositions that may be made respecting the nature and law of the resistance exerted by the particles of matter on the vibrations of the æthereal fluid.

18. The first supposition we shall make is this; that the resistances do not interfere with the principle of the superpo-sition of small motions, but that this principle holds good for the æthereal vibrations in all cases, whether they take place in vacuum or in the interior of transparent substances. In this supposition we are fully warranted by experiment, and we may proceed upon it with confidence.

We have already, in article 16, stated two consequences which result from the supposition here stated; the first of them is obvious; the second we now proceed to prove; it is this, that

$$\frac{T}{v} = (C_1 - C_3\,n^2 + C_5\,n^4 - \,.....\,)$$
$$\frac{N}{v} = n\,(C_2 - C_4\,n^2 + C_6\,n^4 - \,.....\,),$$

where C_1, C_2, C_3, &c. are certain constants depending simply upon the constitution of the æther and of the transparent sub-stance in which it is.

19. Assuming the principle of the superposition of small motions to hold, we may resolve the vibratory motion into two components, one parallel to the axis of x, and represented by ξ, the other parallel to the axis of y, and represented by η. We may determine the laws of propagation of each compo-nent vibratory motion separately and independently of the other; and then, by superposing the two components, the na-ture and laws of which we have thus determined, we arrive at the true result.

Proceeding in this manner, let us first suppose that the whole vibratory motion takes place parallel to the axis of x; then η will be zero, and the resistance will act parallel to the axis of x, and can only depend upon the velocity $\frac{d\xi}{dt}$ and its differential coefficients $\frac{d^2\xi}{dt^2}$, $\frac{d^3\xi}{dt^3}$, &c. &c. Now it is easy to show that the principle of the superposition of small motions cannot hold, unless the forces which are brought into action

upon each elementary slice of the æthereal fluid be *linear* functions of ξ and its differential coefficients. Hence the resistance must be a function of the form

$$C_1 \frac{d\xi}{dt} + C_2 \frac{d^2\xi}{dt^2} + C_3 \frac{d^2\xi}{dt^3} + \&c.,$$

where C_1, C_2, C_3, &c. are certain constants depending upon the constitution of the æther, and of the transparent substance in which it is.

Furthermore, we may assume that the slices vibrate according to the cycloidal law, which gives

$$\frac{d^2\xi}{dt^2} = -n^2 \xi.$$

Hence the expression for the resistance becomes

$$(C_1 - C_3 n^2 + C_5 n^4 \dots) \frac{d\xi}{dt} + (C_2 - C_4 n^2 + C_6 n^4 \dots) \frac{d^2\xi}{dt^2},$$

or

$$p \frac{d\xi}{dt} + q \frac{d^2\xi}{dt^2} = X \text{ suppose,}$$

where, for brevity, we have put

$$C_1 - C_3 n^2 + C_5 n^4 \dots = p, \quad C_2 - C_4 n^2 + C_6 n^4 \dots = q.$$

In exactly the same way we may show that the resistance parallel to the axis of y is

$$p \frac{d\eta}{dt} + q \frac{d^2\eta}{dt^2} = Y \text{ suppose.}$$

Now

$$T = X \frac{1}{v} \frac{d\xi}{dt} + Y \frac{1}{v} \frac{d\eta}{dt},$$

$$N = -X \frac{1}{v} \frac{d\dot\eta}{dt} + Y \frac{1}{v} \frac{d\xi}{dt}.$$

Hence we find that

$$T = \frac{p}{v} \left\{ \left(\frac{d\xi}{dt}\right)^2 + \left(\frac{d\eta}{dt}\right)^2 \right\} + \frac{q}{v} \left\{ \frac{d^2\xi}{dt^2} \frac{d\xi}{dt} + \frac{d^2\eta}{dt^2} \frac{d\eta}{dt} \right\},$$

$$N = \frac{q}{v} \left(\frac{d^2\eta}{dt^2} \frac{d\xi}{dt} - \frac{d^2\xi}{dt^2} \frac{d\eta}{dt} \right),$$

which formulæ are evidently equivalent to

$$\left. \begin{array}{l} T = pv + q \dfrac{dv}{dt} \\[2mm] N = q \dfrac{v^2}{\rho} \end{array} \right\} \quad . \quad . \quad . \quad . \quad . \quad (3.)$$

where ρ is the radius of curvature of the curve of vibration, whatever it be.

Suppose now that the curve of vibration is a circle, or in

2 M 2

other words, that the light is circularly polarized, then we
have

$$\xi = u \cos (n t - k z), \qquad \eta = u \sin (n t - k z),$$
$$v = n u, \qquad \rho = u.$$

Therefore the formulæ just obtained give us

$$\frac{T}{v} = p, \qquad\qquad \frac{N}{v} = n q,$$

or

$$\left. \begin{array}{l} \dfrac{T}{v} = (C_1 - C_3 n^2 + C_5 n^4 \dots) \\[2mm] \dfrac{N}{v} = n (C_2 - C_4 n^2 + C_6 n^4 \dots) \end{array} \right\}, \quad \cdot \quad \cdot \quad \cdot \quad (4.)$$

which was to be proved.

20. We may here remark that these values of $\frac{N}{v}$ and $\frac{T}{v}$
are independent of the amplitude of vibration (u). This shows
that the integration of the equations in article 10 is not merely
approximate, but exact. See also the remarks made in article
13.

21. By the results thus obtained the formulæ (2.) become

$$2 A \frac{k^2}{n^2} = \sqrt{(1+q)^2 + \frac{p^2}{n^2}} + (1+q),$$
$$2 A \frac{h^2}{n^2} = \sqrt{(1+q)^2 + \frac{p^2}{n^2}} - (1+q).$$

Now, since $\frac{T}{v}$ must be very small compared with $n + \frac{N}{v}$ (see
article 11), $\frac{p}{n}$ must be very small compared with $(1+q)$;
hence we have, approximately,

$$A \frac{k^2}{n^2} = 1 + q, \qquad\qquad A h^2 = \frac{p^2}{4(1+q)}.$$

Hence, the length of the luminous wave being λ, the time
of vibration being τ, and therefore

$$\lambda = \frac{2 \pi}{k}, \quad \tau = \frac{2 \pi}{n}, \text{ and } \therefore \frac{k}{n} = \frac{\tau}{\lambda},$$

we find that

$$\frac{1}{\lambda^2} = \frac{1}{A \tau^2} \left\{ 1 + C_2 - C_4 \left(\frac{2 \pi}{\tau} \right)^2 + C_6 \left(\frac{2 \pi}{\tau} \right)^4 - \dots \right\},$$

which formula shows the dependence of the length of the wave
upon the time of vibration, *i. e.* upon the colour.

Again, if a^2 be the intensity of the vibration at any parti-

cular point of the transparent substance, and u^2 the intensity when the wave has traversed any thickness z of the substance, we have

$$u = a\,e^{-hz}$$

and

$$h^2 = \frac{1}{4\,\mathrm{A}}\frac{\left\{C_1 - C_3\left(\dfrac{2\,\pi}{\tau}\right)^2 + C_5\left(\dfrac{2\,\pi}{\tau}\right)^4 \dots\right\}^2}{1 + C_2 - C_4\left(\dfrac{2\,\pi}{\tau}\right)^2 + C_6\left(\dfrac{2\,\pi}{\tau}\right)^4 \dots},$$

which formulæ show the absorbing power of the transparent substance, and its dependence upon the colour.

22. It would be useless to attempt any experimental confirmation of these formulæ, because they must be necessarily imperfect until we find in what manner they are modified by the conditions which must be satisfied at the bounding surface of the transparent substance. The paper in the Cambridge Transactions, vol. viii. p. 3, already quoted, shows the great importance of taking these conditions into account. If my reasoning in that paper be correct, it appears that, what otherwise might pass for a good explanation of absorption, is shown to be really fallacious when the conditions at the surface of separation are taken into account. I have fully investigated the effect of these conditions in the present case, and I find that they introduce some remarkable modifications into the above formulæ, which appear to lead to very important results. These I hope to give in my next communication.

23. The peculiar kind of absorption exercised by a plate of tourmaline may be very easily explained upon the hypothesis of resistance to the vibrations of the æther, as we shall very briefly show.

If we put for X and Y their values obtained in article 19, the equations of motion are

$$(1 + q)\frac{d^2\xi}{dt^2} + p\frac{d\xi}{dt} = \mathrm{A}\frac{d^2\xi}{dt^2},$$

$$(1 + q)\frac{d^2\eta}{dt^2} + p\frac{d\eta}{dt} = \mathrm{A}\frac{d^2\eta}{dt^2}.$$

Now if we suppose the transparent substance to be no longer uniform in all directions, but crystallized, and if we take the axes of coordinates, so that they shall coincide with the axes of symmetry, these equations of motion will still hold; only we must suppose that the p, q and A in the second equation are different from those in the first, as is quite manifest. Consequently, when we integrate each of these equations (just as in article 10), we shall find that the h (which may be called the index of absorption) is not the same quantity for ξ that it

is for η, which explains the peculiar nature of the absorption of tourmaline.

24. To explain the rotation produced by certain fluids in the plane of polarization of a ray transmitted through them, it is necessary to show that the velocity of propagation of circularly polarized light in the fluid is different for right-handed and left-handed rotation. How the fluid can have this effect on light, no matter in what direction it is transmitted through it, is a very great difficulty, and has not been explained so far as I know. I think however that this difficulty may be got over by supposing that the particles of the fluid are compound, each being a group of other particles arranged in such a manner that any section of one of these groups is something like what is represented in the annexed figure (fig. 1). These groups are supposed to be perfectly similar in all respects, and symmetrically posited; so that, if any line be drawn through the centre of one of the groups, not only that group, but the whole system of groups are placed symmetrically about that line. A

Fig. 1.

little consideration will show that the particles composing any group may be so arranged that, if we represent those which lie in any particular section of that group in a figure as above, they will appear to be as it were twisted from right to left, and form intersecting lines of particles, each in the shape ${\cal C}$. In the same manner the particles might be arranged in such a manner that those in any section would appear to be twisted from left to right, as in fig. 2, and form intersecting lines of particles in the shape ${\cal S}$.

Let A (fig. 3) represent any one of the compound particles of the transparent substance, such suppose as is represented in fig. 2; and let A B C be the circle which any point of the

Fig. 2.
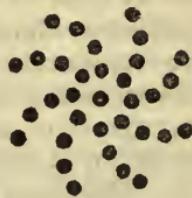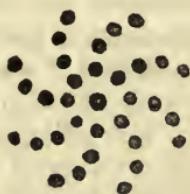
Fig. 3.

æthereal fluid describes (the vibrations being circular). Then,

if the rotation be from right to left, it is clear, from the peculiar shape of the particle A, that there will be a greater resistance offered to the motion of the æthereal fluid by the upper portion (p) of the particle A than by the lower portion (q). Consequently there will be a greater accumulation of resisted æther on the side p than on the side q. On the contrary, it is clear that, if the rotation be from left to right, the reverse will be the case, and then there will be a greater accumulation on the side q than on the side p. Now there are two resistances acting upon each element of the æthereal fluid, as we have already shown, one *tangential* opposing the motion of the element along its circular path, the other *normal*, opposing the normal motion of deflexion of the element. In the present case it is clear that the tangential resistance will be the same whether we suppose the rotation to be from right to left or from left to right, for in both cases the particle A is similarly situated with respect to the direction of motion. But not so with the normal resistance, for, the normal motion always taking place from p towards q, and the accumulation of resisted fluid being greater on the side p when the rotation is from right to left, and greater on the side q when the rotation is from left to right, it is evident that the normal resistance will be greater when the rotation is from right to left than when it is in the opposite direction. Hence we may conclude, in the case of a transparent substance whose particles are constituted as is represented in fig. 2, that T will be the same for right-handed and for left-handed circularly polarized light, but that N will be greater for the former kind of light than for the latter.

In like manner, when the particles are constituted as is represented in fig. 1, the same will be true, only N will be greater for left-handed than for right-handed circularly polarized light.

Now we have shown in article 11, that $\dfrac{T}{v}$ must in general be very small compared with $n + \dfrac{N}{v}$, and we may also conclude that $\dfrac{N}{v}$ is small compared with n, except when the resistance produces a very considerable effect upon the velocity of propagation, which of course cannot be the case in substances of moderate refractive and dispersive power. Hence, by the formulæ (2.) in article (17.), we have very nearly

$$A\,k^2 = n\left(n + \frac{N}{v}\right),$$

$$A\,h^2 = \frac{1}{4}\left(\frac{T}{v}\right)^2.$$

Consequently, by what has been just proved respecting N and T, k will be different for right-handed and left-handed circularly polarized light, but h will not be different, at least will differ very little. In other words, the two kinds of light will be propagated with different velocities, but will not be unequally absorbed; which is a general explanation of the peculiar effect of certain fluids upon circularly polarized light, and by consequence, upon plane polarized light.

We have assumed here that $\frac{N}{v}$ is small compared with n, but it will do just as well to suppose that the absorbing power of the medium is very small, in which case h may be regarded as zero. All that is necessary is, that the two kinds of light be not unequally absorbed by the substance, and yet be propagated with different velocities.

We have thus shown that there is a certain hypothesis on which the peculiar effects of certain fluids upon polarized light may be generally accounted for. We do not however venture to put this forward as the true hypothesis, as it is quite conceivable that some other cause besides that we have supposed might make N different for the two kinds of light, and h either not different or extremely small. Since we must conceive the particles of the fluid to be symmetrically posited in all directions, I think that the cause, whatever it be, must be something peculiar in the constitution of each individual particle, which in some manner affects the resistance brought into action upon the æthereal fluid.

Balham, Surrey, November 11, 1844.   M. O'BRIEN.

[To be continued.]

LXXXV. *Proceedings of Learned Societies.*

ZOOLOGICAL SOCIETY.

Feb. 13, 1844. "ADDITIONAL Measurements of Blood-corpuscles or Red Particles of Mammalia and Birds," by George Gulliver, Esq., F.R.S., No. 2*.

The measurements are expressed in vulgar fractions of an English inch, according to the practice which I have always adopted. As the numerator is invariably 1, it is omitted throughout, the denominators only being printed.

* No. 1 will be found in the Proceedings of the Zoological Society, No. cxix., Dec. 13, 1842, page 190.

In each instance the measurements of the common-sized discs are first set down; a space is then left; the small- and large-sized discs are next noted; and lastly, the average deduced from the preceding numbers is placed beneath the line. Of the oval blood-discs, the long diameter is denoted by the letters L.D., and the short diameter by S.D. The blood was taken from adult living animals, unless stated to the contrary.

MAMMALIA.

Feræ.

Tricoloured Fox (*Canis cinereoargenteus*, Schreb.).

3555
4000

5333
2900

———

3761

Blood from a prick of the tail.

Indian Fox (*Canis Bengalensis*, Shaw).

Same as the preceding.

Indian Tiger-cat (*Felis Bengalensis*, Desm.).

4600
4570

6400
3200

———

4419

Blood from a prick of the upper lip.

Ruminantia.

Stanley Musk Deer (*Moschus Stanleyanus*, Gray).

Measurements detailed in the Proceedings of the Zoological Society, May 9, 1843, page 66. I have since made another observation, which agrees with the former one in showing that the blood-discs of this animal resemble in minuteness those of the Napu Musk Deer.

Virginian Deer (*Cervus Virginianus*, Ray).

5450

5333
5000

7110
3555

———

5036

Blood from a prick of the lip.

Male Ibex (*Capra Caucasica*, Guld).

6858
7110

10666
5333

———

7045

The average is almost exactly the same as in my first measurements of the blood-discs of this animal.

Blood from a prick of the ear.

Female Cashmere Goat (*Capra Hircus*, var.).

6400
6665

8000
5333

———

6466

The average is very nearly the same as in my first measurement of the blood-discs of this animal. See Appendix to Gerber's Anatomy, p. 46.

Blood from a prick of the ear.

Female Kid, twelve days old, bred between the Ibex and Goat just mentioned.

8000
7275

6000
5333
4500

10666
4000

5918

Blood from a prick of the ear.

The preceding measurements confirm my former observations, published in the Proceedings of the Zoological Society, Aug. 9, 1842, p. 107,—that the blood-discs of the Ibex are slightly smaller than those of the Goat.

The Kid between these animals appears to have generally larger and more variously-sized blood-discs than either of the parents.

The observations were favourably made for comparison, because the blood of the three animals was taken and examined from the same part, on the same day and hour, and under the same circumstances.

RODENTIA.

Hackee Squirrel (*Sciurus Listeri*, Ray).
4000
3800
5333
3200

3948

Blood from a vein of the ear.

Quebec Marmot (*Arctomys Empetra*, Schreb.).
3600
3330
4570
2900

3503

Blood from a prick of the lip.

Harvest Mouse (*Mus messorius*, Shaw).
4000

4570
6400
3200

4268

Thus this little animal, the smallest of the British Mammalia, has blood-corpuscles larger than those of the horse, as may be seen by comparing the measurements now given with those of the blood-corpuscles of the horse, published in my Appendix to Gerber's Anatomy, p. 43; and a reference to the dimensions of the blood-corpuscles of numerous Rodentia detailed in the same work, p. 47–50, will show that the corpuscles of the Harvest Mouse are rather smaller than those of any animal yet examined of this order.

The blood of the Harvest Mouse was obtained from the heart, in one case about twelve, and in another twenty-six hours after death, in cool weather.

For opportunities of examining these animals, I am indebted to the kindness of Mr. Griffith and Mr. Prince.

Canada Porcupine (*Erethizon dorsatum*, F. Cuv.).
3555
3428
3200
4572
2666

3380

Blood from a prick of the skin of the nose.

Beaver (*Castor Fiber*, Linn.).
3555
3303
3200
3000
5000

2666
––––
3325

Blood from a prick of the nose of a female about half-grown.

Marsupiata.

Kangaroo Rat (*Hypsiprymnus setosus*, Ogilby).

4000

5333
3200
––––
4000

Blood from a prick of the tail.

The corpuscles are slightly smaller than those of many other animals of the same order, and resemble in size the corpuscles of the Viverrine and Mauge's Dasyure. See my measurements of the blood of discs of Marsupiata, Dublin Medical Press, Nov. 27, 1840; London and Edin. Phil. Magazine, Dec. 1st, 1840; and Proc. Zool. Soc., June 8, 1841, p. 49.

AVES.

Omnivoræ.

Blue Jay (*Garrulus cristatus*, Vieill.).

L.D.	S.D.
2000	3200
2900	4800
1600	3000
2041	3512

Blood from a vein of the pinion.

Insectivoræ.

Rufous Mocking-bird (*Orpheus rufus*).

L.D.	S.D.
2286	3555
2900	4800
1777	3000
2231	3646

Blood from a vein of the wing.

Granivoræ.

Great Titmouse (*Parus major*, Linn.).

L.D.	S.D.
2133	4000
2000	
2900	4800
1777	3200
2132	3892

Blood from a vein of the wing.

Blue Grosbeak (*Loxia cærulea*, Linn.).

L.D.	S.D.
2286	3555
2900	5333
1895	3000
2290	3740

Blood from a vein of the thigh of a female.

Whidah Bird (*Vidua paradisæa*, Cuv.).

L.D.	S.D.
2000	3555
1777	
2900	5333
1684	3000
1998	3740

Nuclei.

| 3555 | 10666 |

Blood from a vein in the wing.

Columbæ.

Crowned Pigeon (*Columba leucocephala*, Ray).

L.D.	S.D.
2000	3555
2900	4800

1777	3000
---	---
2132	3646

Blood from a vein of the pinion.

Moustache Pigeon (*Columba mystacea*, Temm.).

L.D.	S.D.
2000	3200
2900	4800
1714	3000
---	---
2100	3512

Blood from a vein of the pinion.

GALLINÆ.

Bonham's Partridge (*Perdix Bonhami*, Fraser).

L.D.	S.D.
2000	3200
1895	3555
2400	4000
1600	2666
---	---
1933	3282

Nuclei.

4570	10666

Blood from a vein of the thigh of a female.

GRALLATORES.

Scarlet Ibis (*Ibis ruber*, Lacep.).

L.D.	S.D.
2000	3200
1777	
2666	4500
1600	2400
---	---
1948	3153

Blood from a vein of the wing.

Lesser Bittern (*Ardea minuta*, Linn.).

L.D.	S.D.
2000	4000
1895	
2400	5000
1777	3000
---	---
1993	3827

Blood from a vein of the wing.

March 26.—A communication was made by Dr. Falconer, conveying the substance of a paper by Capt. Cautley and himself on the osteological characters and palæontological history of the *Colossochelys Atlas*, a fossil tortoise of enormous size, from the tertiary strata of the Sewalik hills in the north of India—a tertiary chain apparently formed by the detritus of the Himalaya mountains.

A great number of huge fragments, derived from all parts of the skeleton except the neck and tail, were exhibited on the table, illustrative of a diagram by Mr. Scharf of the animal restored to the natural size.

The communication opened with a reference to the reptilian forms discovered in the fossil slate, among which colossal representatives have been found of all the known tribes, such as the *Iguanodon, Megalosaurus, Labyrinthodon*, &c., besides numerous forms of which no living analogues exist, such as the *Enaliosaurian* reptiles and *Pterodactyles*. No fossil *Testudinata* remarkable either for size or deviation from existing forms, have hitherto been found in the fossil state. The *Colossochelys* supplies the blank in the first respect, while it differs so little from the land-tortoises in the general construction of its osseous frame as hardly to constitute more than a subgenus of *Testudo*.

The plastron or sternal portion of the shell affords the chief distinctive character. The episternal portion in the adult is six and a half inches thick, and contracted into a diameter of eight inches, bifid

at the apex, and supplied with a thick cuneiform keel on its inferior side : this keel constitutes one of the principal features in the fossil. The entosternal portion exhibits exactly the form of *Testudo*, the same being the case with the xiphiosternal or posterior portion. The plastron in the adult animal was estimated to be nine feet four inches long.

The carapace or buckler of the shell coincides exactly with the general form of the large land-tortoises, of which it exhibits only a magnified representation, flattened at the top and vertical at the sides, with the same outline and recurved margin. The shell was estimated to have been twelve feet three inches long, eight feet in diameter, and six feet high.

The extremities were described as constructed exactly as in the land-tortoises, in which the form of the femur and humerus is marked by peculiar characters. These bones in the fossil were of a huge size, corresponding to the dimensions of the shell. The ungueal bones indicated a foot as large as that of the largest Rhinoceros. The humerus was more curved, and the articulating head more globular and deeper in the fossil, from which it was inferred that it had a stronger articulation, greater rotation, and that the *Colossochelys* was enabled to bring its anterior extremities more under its weight than is the case with existing tortoises.

The affinities with *Testudo* shown in the shell and extremities were found to hold equally good in the construction of the head, of which a comparatively small-sized specimen, inferred to have belonged to a young or half-grown *Colossochelys*, was exhibited. The head of the adult to correspond with the dimensions of the shell, and according to the proportions furnished by a large *Testudo Indica*, was deduced to have been two feet long.

There were no ascertained cervical vertebræ to afford direct evidence as to the length of the neck, which was constructed in the diagram relatively to the proportions of *Testudo Indica*. The entire length of the *Colossochelys Atlas* was inferred to have been about eighteen feet, and that it stood upwards of seven feet high.

The generic name given by the discoverers has reference to the colossal size of the fossil (κυλοσσὸς et χέλυς), and the specific one to its fitting representation of the mythological tortoise that sustained the world, according to the systems of Indian cosmogony.

May 14.—The conclusion of the paper by Dr. Falconer and Captain Cautley on the Gigantic Fossil Tortoise of India was then read :—

" On a former meeting we went through the anatomical characters presented by the remains of the *Colossochelys Atlas*. Commencing with the plastron, we traced the modifications of form through the costal elements of the carapace and the dorsal vertebræ, all of which bear the closest resemblance to the ordinary type of the Chersite Chelonians, or true land tortoises. A like result followed the examination of the extremities, which, as exhibited in the remains of the humerus, femur and ungueal phalanges, were seen to be constructed exactly on the plan of *Testudo*, with columnar legs and truncated club-shaped feet, as in the proboscidean Pachydermata. The same

direction of affinity was observed throughout the conformation of the head. The only portions of the skeleton from which more or less direct evidence was not derived, were the neck and tail vertebræ, of which there were no specimens in the collection. The general result of the examination showed that the *Colossochelys Atlas* was strictly a land tortoise in every part of its bony frame; and the impressions of the horny scutes proved the like in regard to the arrangement of its dermal integument.

" The principal distinctive characters were found in the sternum, which is enormously thickened at its anterior extremity, along the united portion of the episternal bones, and contracted into a narrow neck, so that the width of the combined episternals does not much exceed their thickness : this thickened portion bears on its under side a deep massive cuneiform keel, which terminates upon the commencement of the entosternal piece. There is more or less thickening of this part in all the species of *Testudo*, and the amount of it is very variable in different individuals of the same species ; but there is nothing approaching the same degree of contraction in reference to the thickness, nor aught like a developed keel, in any of the existing land tortoises which we have either had an opportunity of examining, or seen described in systematic works on the tribe. The keel in the fossil is feebly shown in the young animal, but strongly marked in the adult. Conceiving that generic distinctions are only legitimate in the case of well-defined modifications affecting some of the leading characters in the organization of an animal, we do not consider ourselves warranted in attaching a higher systematic importance to the *Colossochelys* than as a subgenus of *Testudo*, which may technically be defined thus (the distinction resting mainly on the form of the sternum) :—

Subgen. Colossochelys.

Testa solida, immobilis, sterno anticè in collum valdè incrassatum,
*   subtùs carinâ crassâ cuneiformi instructum, angustato.* Testudo
terrestris, staturâ et mole ingenti (inde nomen κολοσσὸς et χέλυς)
sui tribus prodigium ! Olim in Indiæ orientalis provinciis sep-
tentrionalibus degebat.

" *Colossochelys Atlas.*—The first fossil remains of this colossal tortoise were discovered by us in 1835 in the tertiary strata of the Sewalik Hills, or Sub-Himalayahs skirting the southern foot of the great Himalayah chain. They were found associated with the remains of four extinct species of Mastodon and Elephant, species of Rhinoceros, Hippopotamus, Horse, Anoplotherium, Camel, Giraffe, Sivatherium, and a vast number of other Mammalia, including four or five species of Quadrumana. The Sewalik fauna included also a great number of reptilian forms, such as crocodiles and land and freshwater tortoises. Some of the crocodiles belong to extinct species, but others appear to be absolutely identical with species now living in the rivers of India : we allude in particular to the *Crocodilus longirostris*, from the existing forms of which we have been unable to detect any difference in heads dug out of the Sewalik Hills. The same result

applies to the existing *Emys tectum,* now a common species found in all parts of India. A very perfect fossil specimen, presenting the greater part of the evidence of the dermal scutes, is undistinguishable from the living forms, not varying more from these than they do among each other. Prof. Thomas Bell, the highest living authority on the family, after a rigid examination, confirms the result at which we had arrived, that there are no characters shown by the fossil to justify its separation from the living *Emys tectum.* There are other cases which appear to yield similar results, but the evidence has not yet been sufficiently examined to justify a confident affirmation of the identity at present.

" The remains of the *Colossochelys* were collected during a period of eight or nine years along a range of eighty miles of hilly country : they belong in consequence to a great number of different animals, varying in size and age. From the circumstances under which they are met with, in crushed fragments, contained in elevated strata which have undergone great disturbance, there is little room for hope that a perfect shell, or anything approaching a complete skeleton, will ever be found in the Sewalik Hills. It is to be mentioned, however, that remains of many of the animals associated with the *Colossochelys* in the Sewalik Hills have been discovered along the banks of the Irrawaddi in Ava, and in Perim Island in the Gulf of Cambay, showing that the same extinct fauna was formerly spread over the whole continent of India.

" This is not the place to enter upon the geological question of the age of the Sewalik strata ; suffice it to say, that the general bearing of the evidence is that they belong to the newer tertiary period. But another question arises : ' Are there any indications as to when this gigantic tortoise became extinct ? or are there grounds for entertaining the opinion that it may have descended to the human period ?' Any *à-priori* improbability, that an animal so hugely disproportionate to existing species should have lived down to be a contemporary with man, is destroyed by the fact that other species of Chelonians which were coeval with the *Colossochelys* in the same fauna, have reached to the present time ; and what is true in this respect of one species in a tribe, may be equally true of every other placed under the same circumstances. We have as yet no direct evidence to the point, from remains dug out of recent alluvial deposits ; nor is there any historical testimony confirming it ; but there are traditions connected with the cosmogonic speculations of almost all Eastern nations having reference to a tortoise of such gigantic size, as to be associated in their fabulous accounts with the elephant. Was this tortoise a mere creature of the imagination, or was the idea of it drawn from a reality, like the *Colossochelys* ?

" Without attempting to follow the tortoise tradition through all its ramifications, we may allude to the interesting fact of its existence even among the natives of America. The Iroquois Indians believed that there were originally, before the creation of the globe, six male beings in the air, but subject to mortality. There was no female among them to perpetuate their race ; but learning that there

was a being of this sort in heaven, one of them undertook the dangerous task of carrying her away. A bird (like the Garūda of Vishnoo or the Eagle of Jupiter) became the vehicle. He seduced the female by flattery and presents : she was turned out of heaven by the supreme deity, but was fortunately received upon the back of a tortoise, when the otter (an important agent in all the traditions of the American Indians) and the fishes disturbed the mud at the bottom of the ocean, and drawing it up round the tortoise formed a small island, which increasing gradually became the earth. We may trace this tradition to an Eastern source, from the circumstance that the female is said to have had two sons, one of whom slew the other ; after which she had several children, from whom sprung the human race.

" In this fable we have no comparative data as to the size of the tortoise, but in the Pythagorean cosmogony the infant world is represented as having been placed on the back of *an elephant, which was sustained on a huge tortoise.* It is in the Hindoo accounts, however, that we find the fable most circumstantially told, and especially in what relates to the second Avatar of Vishnoo, when the ocean was churned by means of the mountain Mundar placed on the back of the king of the tortoises, and the serpent Asokee used for the churning-rope. Vishnoo was made to assume the form of the tortoise and sustain the created world on his back to make it stable. So completely has this fable been impressed on the faith of the country, that the Hindoos to this day even believe that the world rests on the back of a tortoise. Sir William Jones gives the following as a translation from the great lyric poet Jyadeva : ' The earth stands firm on thy immensely broad back, which grows larger from the callus occasioned by bearing that vast burden. O Cesava ! assuming the body of a tortoise, be victorious ! Oh ! Hurry, Lord of the Universe ! '

" The next occasion in Indian mythology where the tortoise figures prominently is in the narratives of the feats of the bird-demigod ' Garūda,' the carrier of Vishnoo. After stating the circumstances of his birth, and the disputes between his mother Vinūta and ' Kudroo,' the mother of the serpent, it is mentioned that he was sent on an expedition to bring ' Chundra' the moon, from whom the serpents were to derive the water of immortality. While pursuing his journey, amidst strange adventures, Garūda met his father Kŭshgŭfa, who directed him to ' appease his hunger at a certain lake, where *an elephant and tortoise were fighting*. The body of the tortoise was eighty miles long—the elephant's 160. Garūda with one claw seized the elephant—with the other the tortoise, and perched with them on a tree 800 miles high.' He is then, after sundry adventures, stated to have fled to a mountain on an uninhabited country, and finished his repast on the tortoise and elephant.

" In these three instances, taken from Pythagoras and the Hindoo mythology, we have reference to a gigantic form of tortoise, comparable in size with the elephant. Hence the question arises, are we to consider the idea as a mere fiction of the imagination, like the Minotaur and the chimæra, the griffin, the dragon, and the cartazonon, &c., or as founded on some justifying reality ? The Greek and

Persian monsters are composed of fanciful and wild combinations of different portions of known animals into impossible forms, and, as Cuvier fitly remarks, they are merely the progeny of uncurbed imagination; but in the Indian cosmogonic forms we may trace an image of congruity through the cloud of exaggeration with which they are invested. We have the elephant, then as at present, the largest of land animals, a fit supporter of the infant world; in the serpent Asokee, used at the churning of the ocean, we may trace a representative of the gigantic Indian python; and in the bird-god Garūda, with all his attributes, we may detect the gigantic crane of India (*Ciconia gigantea*) as supplying the origin. In like manner, the *Colossochelys* would supply a consistent representative of the tortoise that sustained the elephant and the world together. But if we are to suppose that the mythological notion of the tortoise was derived, as a symbol of strength, from some one of those small species which are now known to exist in India, this congruity of ideas, this harmony of representation would be at once violated; it would be as legitimate to talk of a rat or a mouse contending with an elephant, as of any known Indian tortoise to do the same in the case of the fable of Garūda. The fancy would scout the image as incongruous, and the weight even of mythology would not be strong enough to enforce it on the faith of the most superstitious epoch of the human race.

" But the indications of mythological tradition are in every case vague and uncertain, and in the present instance we would not lay undue weight on the tendencies of such as concern the tortoise. We have entered so much at length on them on this occasion, from the important bearing which the point has on a very remarkable matter of early belief entertained by a large portion of the human race. The result at which we have arrived is, that there are fair grounds for entertaining the belief as probable that the *Colossochelys Atlas* may have lived down to an early period of the human epoch and become extinct since :—1st, from the fact that other Chelonian species and crocodiles, contemporaries of the *Colossochelys* in the Sewalik fauna, have survived; 2nd, from the indications of mythology in regard to a gigantic species of tortoise in India.

" Some of the bones were analysed with great care by Mr. Middleton, and yielded a large proportion of fluorine, the constituents being,—

Phosphate of lime	64·95
Carbonate of lime	22·36
Fluoride of calcium	11·68
Oxide of iron	1·00
A trace of chloride of soda.	——
	99·99

" Other Sewalik fossil bones were at the same time subjected to analysis, such as the *Mastodon elephantoides*, *Camelus sivalensis*, Horse, Ruminants, &c., and the whole of them yielded similar results, with a proportion of fluoride of calcium varying from 9 to 11 per cent. This is much above the usual quantity found in fossil bones; the utmost that has been met with having been in bones of the Anoplotherium from the Paris basin, 14 per cent."

June 11.—" On the Blood-corpuscles of the Two-toed Sloth, *Bradypus didactylus*, Linn.," by George Gulliver, F.R.S.

From an observation which I have lately made, it results that the Two-toed Sloth is one of the very few animals that has blood-discs considerably larger than those of Man.

The following measurements of the blood-discs of the Sloth are given in vulgar fractions of an English inch :—

$$
\left.\begin{array}{l}
1\text{-}3200 \\
1\text{-}3000 \\
1\text{-}2888 \\
1\text{-}2823 \\
1\text{-}2769 \\
1\text{-}2664 \\
1\text{-}2583
\end{array}\right\} \text{Common sizes.}
$$

$$
\left.\begin{array}{l}
1\text{-}4266 \\
1\text{-}2286
\end{array}\right\} \text{Extremes.}
$$

1-2865　　Average.

M. Mandl* discovered that the blood-corpuscles of the Elephant are the largest at present known belonging to the Mammalia, and I subsequently found that those of the Capybara were, as far as we then knew, next in size, as noticed in my Appendix to Gerber's Anatomy, pages 5, 8, and 50.

But it now appears that the blood-corpuscles of the Sloth are larger than those of the Capybara, and, among mammiferous animals, second only in magnitude to the corpuscles of the Elephant.

For the sake of comparison, some of my measurements of the average size of the largest blood-discs of Mammalia are here set down in the order of the magnitude of the discs, and in vulgar fractions of an English inch.

Elephas Indicus, Cuv.	1-2745
Bradypus didactylus, Linn.	1-2865
Balæna Boops, Auct.	1-3099
Hydrochærus Capybara, Erxl.	1-3216
Phoca vitulina, Linn.	1-3281
Dasypus villosus, Desm.	1-3315
Myopotamus Coypus, Desm.	1-3355
Pithecus Satyrus, Geoff.	1-3383
Dasypus sex-cinctus, Auct.	1-3457

Numerous other measurements are appended to the English version of Gerber's Anatomy.

It has been said that the blood-corpuscles are larger in omnivorous than in herbivorous and carnivorous animals. To the facts which I have elsewhere† shown to be at variance with this opinion, it may be

* Anatomie Microscopique, Paris 1838, Prem. Liv. p. 17. M. Mandl's observation refers to the blood-corpuscles of the African elephant; it was those of the Asiatic species that I examined.

† Appendix to Gerber's Anatomy, p. 4–5.

added that the oviparous Vertebrata, whatever may be the nature of their food, have larger blood-corpuscles than Mammalia, and that the size of the blood-corpuscles of many carnivorous birds exceeds that of the corpuscles of several of the omnivorous species.

Finally, the Two-toed Sloth, which is a purely vegetable feeder, has, excepting the Elephant, the largest blood-corpuscles hitherto observed in any mammiferous animal.

GEOLOGICAL SOCIETY.

November 6, 1844.—First Meeting of the Session. The Presi-dent, Mr. Warburton, in the Chair. A paper was read entitled " Observations on the Geology of some parts of Tuscany," by Mr. W. J. Hamilton, M.P., Sec. G.S.

One of the principal features of the district examined by the author, is the existence of three distinct mountain ridges, extending from N.W. to S.E. by S. parallel to the direction of the main chain of the Apennines, and all belonging to the cretaceous system. The valleys between these ranges are filled with tertiary deposits. Second-ary formations form the greater part of the mountainous district of Tuscany, consisting of beds of sandstone, indurated marls and shales, and compact gray lithographic limestone or scaglia. These some-times alternate with each other, and are variously developed in dif-ferent localities. Fossils are rare in all of them. The tertiary for-mations are both marine and freshwater. The marine tertiaries at-tain a height of nearly 1800 feet in the basin of Volterra, where they consist of beds of blue marl and sandy limestones, capped by shelly limestone. Marine shells are frequent in some of these beds, of which the blue marl is the most extensive, attaining in the locality referred to a thickness of nearly 1000 feet. Selenite abounds in part of it ; also beds of rock-salt and alabaster, extensively worked, the mines of the latter by means of regularly constructed mining galle-ries. The other marine tertiary districts are those of Leghorn, Pog-gebonzi, Sienna, and Val de Chiana. Freshwater tertiaries were noticed in two localities, forming limestones as compact and having the aspect of scaglia, but well characterized by their peculiar organic remains. Extensive post-tertiary formations of calctuff occur in the valley of the Staggia and of Elsa, and the beds are in places more than 100 feet thick. The rock called "Gabbro Rosso" by Savi, Mr. Hamilton considers as a metamorphic rock derived from the altered marls and sandstones of the secondary formation, acted on by the protrusion of igneous rocks of the serpentine class. At the junction of the Serpentine and Gabbro at Monte Catini is found copper ore (a sulphuret) extensively worked. Besides the serpen-tine, the quasi-trachytic rock called Selagite, and the basalts of Radi-cofani, are among the igneous rocks of the district. The author con-cludes with an account of the remarkable boracic acid works at Monte Cerboli, and of the phænomena connected with them.

LXXXVI. *Intelligence and Miscellaneous Articles.*

EXPERIMENTS ON THE TRANSMISSION OF ELECTRIC CURRENTS[*].

PROFESSOR Belli has communicated to me the results of the experiments made during the late Scientific Meeting at Milan, at my suggestion, on the conductibility of the electric fluid by the earth. The two stations were Milan and Monza. In the first experiment, the circuit, which was altogether metallic, was nearly 12,500 metres. In the second, the circuit was partly metallic and partly the earth, that is to say, 12,500 metres of wire and 12,500 metres of earth. In the third, the circuit was altogether metallic, of 25,000 metres.

The pile was a single pair of constant force. The intensities were in the relations of 30 : 27 : 17.

The result was a confirmation of what I had found with less distances.

The first circuit, wholly metallic, which gives 30, is the half of the second in length. This second is composed of the same wire and of the earth, and in this case we find 27. The difference, very small in comparison of the double length of the circuit, would certainly have been found with less distances of earth, for it is due to the change of conductor.

In the third experiment, when the circuit was wholly metallic and of the same length as the second, the difference became very great.

When earth is employed for great distances, not only does the resistance due to the length disappear, but that even of the wire employed is no longer found. I still doubt whether this result be due to chemical actions on the plates. It would be very important for science and for telegraphy that experiments on a greater scale should be made.—*Comptes Rendus,* October 1844.

TRANSMISSION OF SCIENTIFIC JOURNALS, ETC.

The following notices may be useful in regard to the object of the Recommendation of the British Association at the late Meeting at York, " For a representation to Government of the difficulty, delay and expense which attend the transmission of Scientific Journals between the British Isles and foreign countries."

Postal Treaty between Belgium and England.

Art. 30. The postage for periodicals, published in Belgium or in Great Britain, to be paid in advance, viz. in Belgium five centimes ($\frac{1}{2}d$.) for every printed sheet under thirty square decimetres (118 square inches) ; and in England one penny for every work under two ounces ; sixpence for every work above two ounces and not exceeding three ; eighteenpence for every work above three ounces and not exceeding four ; and for every ounce above four and under sixteen, twopence more.

The present treaty to be put in execution on the 1st of December, 1844.

[*] Extract from M. Matteucci's correspondence with M. Belli.

Royal Decree granting free Carriage to the Correspondence of the Academy.

Leopold, King of the Belgians.

To all present and to come, greeting.

Whereas by our Decree of the 8th of November, 1841, which grants freedom of carriage to the official correspondence of the Royal Academy of Medicine. Considering that, from similar motives, there is reason to grant the same privilege to the Royal Academy of Sciences and Belles-lettres of Brussels;

Upon the proposition of our Minister of Public Works; We have decreed and do decree :—

Art. 1. Our Minister of the Interior is authorized to correspond free of carriage, *under seal*, with the office of the Academy of Sciences and Belles-lettres of Brussels and the members of this body individually.

Art. 2. The frank is likewise granted to the correspondence under slip-covers and the counter-sign which the Academy and its Secretary must exchange with each of its Members.

Art. 3. The counter-sign of the Academy shall be used in the name of the body, either by the President or by the regular Secretary, for this purpose.

Our Minister of Public Works is charged with the execution of the present Decree. Given at Brussels the 22nd of December, 1841.

ON OPIAMMON.

M. Wöhler gives this name to a substance which is produced by the changes which opianate of ammonia undergoes; once he obtained this salt in large tables, by the spontaneous evaporation of a solution of opianic acid in ammonia, mixed with alcohol. Opianic acid disappears instantaneously in caustic ammonia, and it absorbs ammoniacal gas with the disengagement of heat. Even though the solution should be evaporated by a very gentle heat, no crystals are formed, but merely a transparent amorphous mass, which becomes milk-white when treated with water, dissolving only partially in it, and leaving a white substance, to which M. Wöhler gives the name of *opiammon*; the ammoniacal salt is completely changed into this compound by heating the dried mass to a little above 212°, as long as ammonia is evolved. It eventually acquires a lemon-yellow colour and becomes insoluble in water; in order to separate the last traces of undecomposed salt, it is sufficient to boil it in water and to filter.

Opiammon is a powder of a pale yellow colour : it would probably be colourless if perfectly pure; it is evidently composed of transparent, crystalline, grumous particles, when examined microscopically. It is insoluble in cold water, but soluble in small proportion in boiling water; this effect is certainly produced as the result of decomposition, for the water acquires a slight acid reaction; when heated under water to 302° F. in sealed tubes, it dissolves completely. The limpid solution is of a yellow colour; on cooling, it deposits crystals of opianic acid, and retains opianate of ammonia

in solution. Alcohol, though slowly, yet dissolves a considerable quantity of opiammon; by spontaneous evaporation some is deposited in crystals, but it is always mixed with opianic acid. When heated it creeps up the sides of the vessel without subliming; when more strongly heated in the open air, it yields the odour of melted opianic acid, and emits a yellow vapour. Diluted acids, even when heated, do nòt alter it. Concentrated sulphuric acid dissolves it even cold, and becomes of an orange-yellow colour; the solution is rendered milky by the addition of water. If it be heated in this state, it becomes clear, and on cooling opianic acid crystallizes; an ammoniacal salt is formed at the same time.

The analyses of opiammon yielded the following results; the azote being determined lime and soda :—

	Experiments.		Equivalents.	Calculation.
	I.	II.		
Carbon	59·92	59·80	40	60·168
Hydrogen ...	4·94	4·82	17	4·248
Azote......	3·74	3·76	1	3·545
Oxygen	31·40	31·62	16	32·039
	100·	100·		100·

The mode of production and the properties of this substance indicate its composition. It can be produced only by the elimination of a portion of the ammonia, and of the elements of the water of the opianate of ammonia; the results of the analyses, and the reactions which this substance presents when heated to 302° in sealed tubes, can be explained only by supposing that 4 equivalents of water, and 1 equivalent of ammonia have been eliminated from the opianate of ammonia, and consequently its empirical formula is $C^{40} H^{17} NO^{16}$.

M. Wöhler states, that although opiammon possesses the properties of the bodies described under the name of *amides*, he is of opinion that it cannot be represented by the formula $NH^2 + C^{40} H^{15} O^{16}$; but this subject he proposes to reconsider; he further remarks, that the hydrogen found by experiment does not agree well with that derived from calculation, which may he thinks be attributed to the hygrometric property of opiammon, or because that which was analysed being yellowish, retained traces of a substance hereafter to be treated of.—*Ann de Ch. et de Phys.*, October 1844.

XANTHOPENIC ACID.

M. Wöhler gives this name to a substance which is produced by the action of opiammon and alkalies on each other; it is an azotized acid, characterized by the yellow colour of its salts.

When caustic potash is poured on opiammon, it is not immediately acted upon, nor does it evolve ammonia. In a short time it dissolves; the solution is of a fine orange-yellow colour, and much ammonia is given out; the same reaction occurs with the alkaline carbonates. When the solution is boiled till the evolution of ammonia ceases, its colour does not alter; in this state it contains opianate and xanthopenate of potash, the acid of which is separated and pre-

cipitated in fine yellow flocculi by hydrochloric acid from the hot so-
lution; this is separated by the filter; the filtered liquid deposits
crystals of opianic acid on cooling.   This method of separation, how-
ever, is not sufficient, for as xanthopenic acid is soluble in hot water,
there always remains some mixed with the opianic acid, which cry-
stallizes and seems to remain combined with it, so that the properties
of the latter are so much modified, that M. Wöhler was inclined to
believe at first that the mixture was a new and distinct acid.   The
opianic acid instead of appearing in small prisms is deposited in
small mammillated masses, of a fine uranium yellow colour ; this
property is always communicated to it when it contains foreign
bodies ; it does not alter even by repeated crystallizations.

It was soon found by M. Wöhler that this substance was opianic
acid coloured by xanthopenic acid, but in so small a quantity that the
composition of the salts of lead and silver was not altered by it ; it
is readily obtained colourless by employing hypochlorite of soda.

M. Wöhler, on account of the small quantity which he possessed,
was able to examine xanthopenic acid but imperfectly.   When pre-
cipitated by an acid it is in yellow flocculi, which when dried is a
yellow powder, presenting a crystalline appearance when examined
by the microscope. It is fusible, dissolves in the alkalies with a yel-
low colour, and treated with a mixture of lime and soda, it yields
ammonia.

Opiammon dissolves readily in hot caustic ammonia, and when the
solution is saturated by hydrochloric acid, colourless opianic acid
only is separated, and it appears that no xanthopenic acid is formed
by the action of ammonia.

Opiammon, treated with an excess of liquid caustic potash, yields
nearly three-fourths of the whole quantity of azote in the state of
ammonia, by the influence of the alkali, and is changed into opianic
and xanthopenic acid, containing one-fourth of the azote of the opi-
ammon.—*Ann. de Ch. et de Phys.*, October 1844.

Examination of the Seeds of Phytelephas macrocarpa *and* micro-
carpa. *By* M. von BAUMHAUER.

M. Baumhauer has submitted the seeds of the above tree*, which
have been for some time brought from Peru into the European market,
to a chemical examination. On account of their hardness and snow-
white colour, for which they have been applied in various ways to
turning, they have been called vegetable ivory. In the unripe state,
Ruiz and Pavon state that they are eaten by the Peruvians.   The
juicy stone-fruits are crowded together on a common stalk ; they are
angular, containing four seeds ; each seed is surrounded with horny
albumen. The walls of the cells are composed of a hard rind, which
are generally still attached to the fruit as it occurs in commerce.

Payen, in his memoir on cellulose, has already analysed the seed-
coats of the *Phytelephas*, after treating them with æther, alcohol,

* [Mr. Connell's examination of these seeds will be found at p. 104, vol. xxiv.
—ED.]

ammonia, acetic acid and water. He found the composition as
follows :—

Carbon..............................	44·14
Hydrogen	6·30
Oxygen	49·56

He, moreover, found that they were converted into dextrine by sul-
phuric acid ; into xyloidine, without the production of any colour, by
nitric acid ; thus they consisted of pure cellulose, containing traces
merely of silica.

Baumhauer concludes from his experiments, that the perisperm of
the above nuts is not, as Payen supposes, pure cellulose mixed with
albumen, two nitrogenous bodies, two fatty matters, silica and salts,
but that another substance is still present, which has a somewhat
different composition to that of cellulose. He proceeded in his ex-
periments in a similar manner to that of Payen. The seed-coats
were separated from the brown rind by a file, then powdered by
using a finer file and sifting, and exhausted by æther, alcohol and
boiling water. The remaining powder, which contained neither
nitrogen nor ash, yielded in three analyses—

	I.	II.	III.
Carbon	44·28	44·44	44·39
Hydrogen	6·32	6·13	6·22
Oxygen	49·40	49·43	49·39

It was now repeatedly boiled with strong acetic acid, washed with
hot water and alcohol, dried at a temperature of 284°–302°, and
again analysed. It gave—

Carbon..............................	43·57
Hydrogen	6·27
Oxygen	50·16

The powder obtained in the first experiment yielded to ammo-
nia a brown matter, precipitable by acetate of lead ; the residue,
after repeatedly washing with water, acetic acid and alcohol, was
of a yellow colour. It was dried at 284°–302°, and burnt with
oxide of copper (I.). Two other portions were treated for a consi-
derable length of time with dilute solution of soda, and after washing,
&c., also analysed (II. III.) ; they yielded—

	I.	II.	III.
Carbon	43·65	45·73	45·59
Hydrogen	6·31	6·32	6·57
Oxygen	50·04	57·95	47·84

After partial evaporation, acetic acid threw down brown flocks from
the soda solution. Ammonia and acetate of lead caused a whitish-
yellow precipitate in the solution.

To separate the two substances, Baumhauer digested the powder of
the seed-shells for several days in cold concentrated solution of pot-
ash ; after dilution with water, the powder was separated by filtra-
tion, washed with hot water, and the whole operation repeated as
long as the potash dissolved anything. The residue was finally ex-
hausted with hot concentrated acetic acid, alcohol and æther.

The ingredient in solution was separated by saturating the alco-

holic solution with acetic acid, washing with cold water, and treating with hot acetic acid, alcohol and æther. By digestion with cold concentrated solution of potash, no decomposition takes place, inasmuch as the residuary powder and the dissolved ingredient are both obtained of a snow-white colour. The powder which remains after treating the seed-coats of *Phytelephas*, when dried at 302°, Baumhauer found to possess the following composition :—

Carbon	43·63	24	43·71
Hydrogen	6·30	42	6·24
Oxygen	50·07	21	50·05

The substance soluble in potash, which the author intends examining more accurately, yields the same composition as starch, from which however, as from inuline, it differs in properties. By decomposing the salt of lead with sulphuretted hydrogen, evaporation, and washing the residue with the solvents mentioned above, it is obtained in a state of purity.—*Ann. der Chem. und Pharm.* xlviii. p. 356.

ON LIMONE. BY DR. C. SCHMIDT.

This substance, which was discovered by Bernays in the pips of lemons and oranges, is probably contained in the seed of all the *Aurantiaceæ.* From the author's investigation it appears that it is not a base, and does not contain any nitrogen. It is obtained in the form of a white crystalline powder; its crystals belong to the rhombic system. This substance is very sparingly soluble in water, æther and ammonia, somewhat more in mineral acids, more easily in alcohol and acetic acid, but most readily in potash, from which solution it is precipitated unaltered by acids. Concentrated sulphuric acid dissolves it with a blood-red colour, from which water precipitates it, as well as from the alcoholic and acetic solutions, unchanged; it is carbonized by heat. When neutralized with carbonate of barytes, no barytes remains in the solution. It crystallizes readily from acetic acid; the crystals are free from chemically-combined acetic acid. The alcoholic solution has a neutral reaction; it neither affords precipitates with chloride of platinum nor with perchloride of mercury, with salts of lead nor with those of silver, potash, barytes and others (the latter must naturally be dissolved in alcohol, as otherwise the pure substance is precipitated by the water of the saline solution). It can be heated to 302° without undergoing change and without any remarkable loss in weight; at a higher temperature it suddenly becomes yellowish, and melts at 471° to a liquid of the same colour, which resembles a melting resin; it resolidifies to an amorphous mass, and even after several days does not present any trace of crystalline structure; it may however be dissolved in acetic acid by long digestion in the warmth, from which it crystallizes in the form and with the properties of the original unmelted substance.

The indifference of this substance towards oxidizing agents is remarkable; concentrated nitric acid dissolves it, especially with the assistance of a little heat; the solution is of a pale yellow, and even after long heating it is precipitated by water from the solution in

an unaltered state. On boiling it with a concentrated solution of bichromate of potash it undergoes no change, nor with free chromic acid, *i. e.* sulphuric acid and chromate of potash, in which solution it floats unaltered even after boiling for several hours. The acetic solution has a strong bitter taste; however, it does not appear to produce any considerable physiological effects. 60 milligrammes, dissolved in acetic acid and taken early in the morning before breaking fast, produced no effect; nothing could be detected in the secretions. 25 millegrammes were given to a dog and 10 millegrammes to a bird, but likewise without any effect.

The elementary composition proved the perfect identity of the substance prepared from lemon and orange pips, and gave as the mean of several analyses, in 100 parts—

Carbon	66·09	42	66·17
Hydrogen	6·55	25	6·55
Oxygen	27·36	12	27·32

The formula is that of phloridzine, dried at $212°$ + 2 equiv. oxygen. Direct experiments with reducing agents, such as sulphurous acid, hydrochloric acid, sulphuretted hydrogen, hydrogen in *statu nascenti*, and the electric current, afforded however by their negative results the proof that this substance does not belong to the phloridzine series, especially as the characteristic decomposition of salicine by chromic acid was wanting. The combining weight could not be ascertained, since it was impossible to obtain combinations of this substance with other bodies.—*Göttinger gel. Anz.*, No. cxxi. 1844.

NEW METHOD OF SILVERING GLASS.

At a meeting of the Chemical Society Mr. Warington described a new method of covering glass, by precipitation, with a coating of metallic silver, the invention of Mr. Drayton. It consists in partially precipitating, and thus neutralizing, to use the inventor's own words, a solution of nitrate of silver by spirit of hartshorn, and adding to the clear solution, after subsidence, oil of cassia previously dissolved in spirits of wine. This compound mixture forms the silvering menstruum, and is to be poured on to the surface of the glass, or into the vessel intended to be silvered, the surfaces having been previously rendered perfectly clean. Oil of cloves dissolved in spirit of wine is then to be gradually dropped over the surface of the silvering solution, or the two solutions may be rapidly mixed and then applied immediately. In the course of about fifteen minutes a faint purple cloud appears, and this gradually spreads through the whole of the solution, and deepens in tint until it becomes opake, when the operation is complete, and a most beautiful mirror is obtained; as thus produced, the reflecting surface is darker in its aspect and more similar to the brilliancy of a very highly polished speculum. The risk of breakage attendant on the usual process, by means of tin foil and mercury, is also avoided, particularly where very large looking-glasses are being constructed, and the great advantage obtained of being enabled to silver uneven surfaces, as of lenses or cut glass.

INDEX TO VOL. XXV.

END OF THE TWENTY-FIFTH VOLUME.

ALERE   FLAMMAM.